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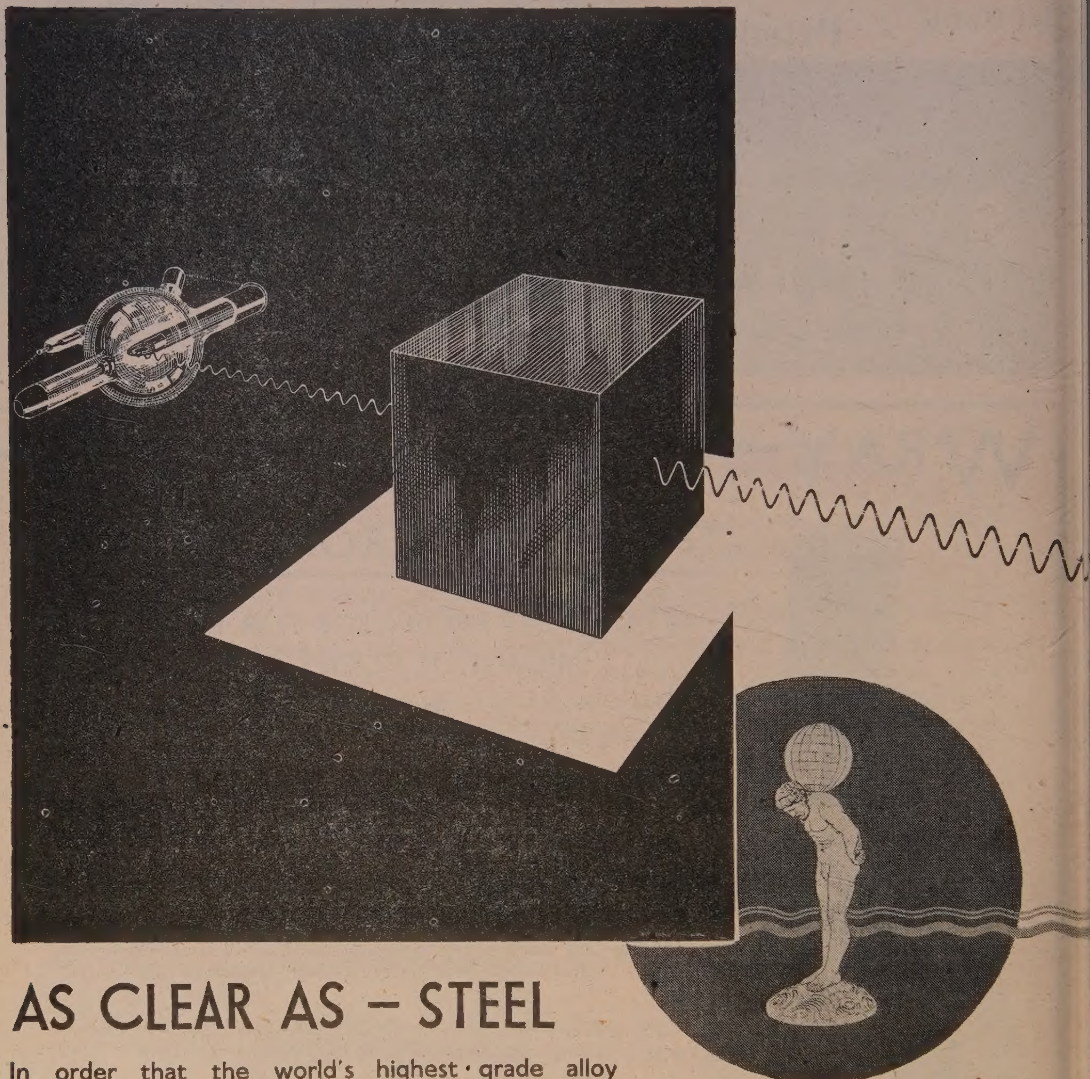

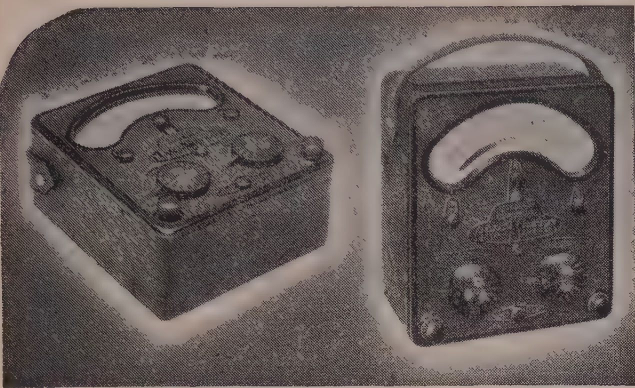

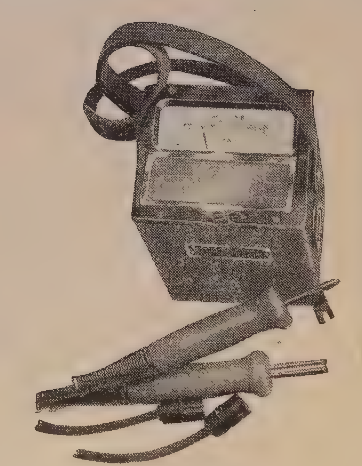

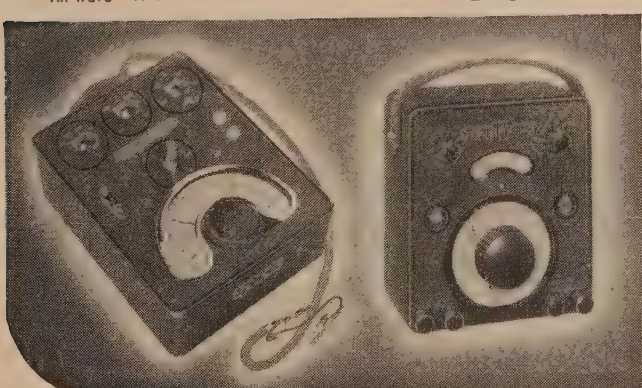

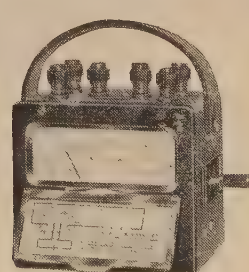

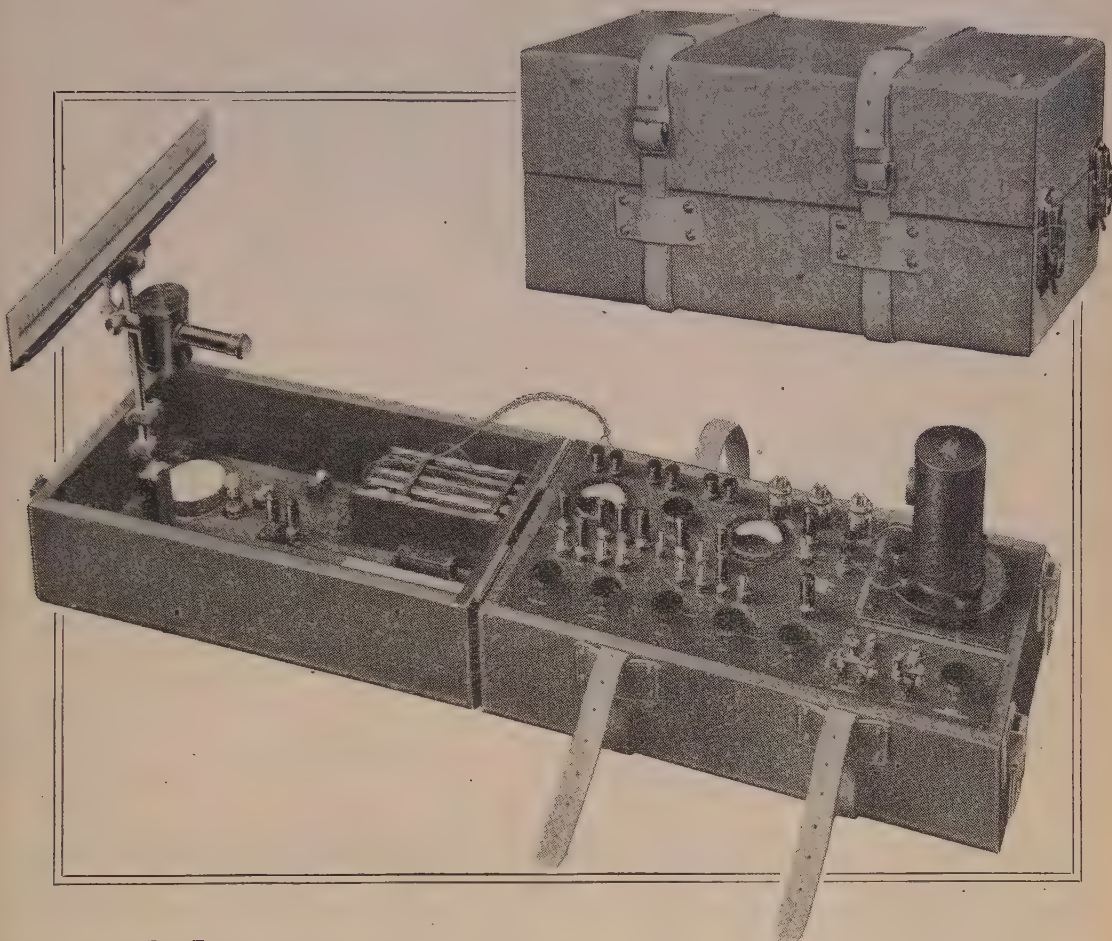

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THE NATURE AND MEASUREMENT OF WHITENESS *

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MS. received 6 *November* 1941

ABSTRACT. The uniqueness of white is discussed from both the visual and colorimetric points of view. The sensitivity of the eye to departures from white are considered, together with the extent to which the visual mechanism compensates for changes in illumination. A short account is given of the instrumental methods of colorimetry most suitable for the measurement of whiteness.

§ 1. INTRODUCTION AND DEFINITIONS

EVERYONE is familiar with the idea of whiteness relating either to a white substance or to a white light. The perfectly white substance is unique, and is thus different from a yellow or blue substance, which may be found in a wide variety of colours depending on the criterion of observation. Perfectly white light cannot be defined objectively, and it has been found necessary to attempt, and very difficult to obtain, agreement on a standard " white " illuminant.

A *perfect white* substance can be defined as one which reflects all light falling on it. It is a corollary that it should possess no absorption of light at any part of the spectrum. A white substance is one which possesses negligible selective absorption of light at different wavelengths and which, therefore, will not make any appreciable change in the colour of light illuminating it, although it may reflect a smaller amount of light than it receives. *Whiteness*, or the state of being white, relates to the colour or selective absorption of a substance rather than to its reflection factor or overall absorption. It may be pointed out that no reflecting substance possesses colour of its own, its colouring properties depending entirely on the selective absorption which it exerts on the light falling on it. The colour name given to such a substance is usually the complement of the colour which is most strongly absorbed.

The reflection factor of a white substance, that is the ratio of reflected to incident light flux, may vary very widely, from 90% for opal glass to 10% for some textiles, and those concerned with each specific material have to agree

* A paper read to the Colour Group on 25 September 1941.

as to the border-line between white and grey. The whiteness is, of course, independent of the intensity of illumination under which the substance is observed.

There is no satisfactory theory to account for selective absorption, but the mechanism of " white " reflection is comparatively simple. A beam of light falling on snow crystals is partly reflected at the crystal surfaces and partly refracted through the crystals to suffer reflection at other surfaces, so that eventually it is nearly all diffusely reflected or scattered, undergoing very little absorption and no transformation. The same is true of a suspension, such as milk, opal glass, fog or paint, in which transparent particles are suspended in a transparent medium of different refractive index and the light is reflected specularly at a multiplicity of interfaces. If the two phases had the same refractive index, the materials would be comparatively transparent, like ice suspended in water. Paper and fabrics are white because they consist of non-absorbing transparent fibres in air, powders are white because they consist of minute crystalline or vitreous particles in air; and it is common experience that their reflection factor is controlled by particle size and by the difference in refractive index, being high for dense crystals such as white lead and titania and decreasing when such crystals are immersed in water or liquids of higher refractive index.

A *white light* has been variously defined as " north sky daylight ", " mean noon sunlight ", or " equi-energy white ", and for the purpose of this paper it is sufficient to say that there is a range of white lights from which each may choose the one most suited to his immediate needs, such as the Planckian distribution at 2360° K. for signal lights (BSS. 623—1940) or the C.I.E. illuminant C for artificial daylight (BSS. 950—1941). The colour of a white light is as real and as important as that of a yellow or blue light (Holmes, 1941) and it may be obtained in as many ways, but the requirement for a standard white illuminant is that the distribution of energy through the spectrum should be smooth and constant. This led to the adoption of equi-energy white (C.I.E. 1931) for theoretical purposes and an incandescent source of light with or without a colour filter for both theoretical and practical purposes. The whiteness of such a source is not dependent on its intensity or brightness, although the visual impression may change.

§ 2. VISUAL EFFECTS

The eye is surprisingly critical in respect of white substances, and small departures from white are described as cream or ivory, whereas a wide range of colours would receive the simple colour names red, green, blue, etc. It seems, almost, that there is an ingrained sense of whiteness which has been sharpened by the frequency with which near-white substances are commonly met and can be compared with surrounding objects, so that the uniqueness of white enables it to be recognized, whereas a particular yellow or blue or even a particular grey calls for an effort of memory. This discrimination does not apply to lights which are commonly called " white ", and there is just as wide

a variation of these as of any other colour, probably because the eye becomes accustomed to changes between sunlight and skylight, electric light, gas light, etc. A white substance can be picked out quite critically in daylight or artificial light, although the actual colour is widely different under the two conditions, and a white substance or a white light will still appear white if the brightness is reduced to about 0·1 foot-lambert where the Purkinje effect is operative but where colour recognition is still fairly critical.

§ 3. COLORIMETRY

The colorimetry of whites is not fundamentally different from that of other colours, but in several ways it is more exacting. Visual matches may be made by additive or subtractive mixing, the former being more flexible but usually less accurate because it will not reproduce the spectral energy distribution of near-white as closely as the latter and is therefore restricted to a much smaller field size than the latter. Departures from white are usually expressed graphically on the C.I.E. chart or R.U.C.S. chart or on some diagram relating specifically to the method of measurement, but in many industrial applications there are three variables (hue, saturation, and reflection factor) which require to be indicated.

The effect of employing different white illuminants may be considerable when examining coloured substances (Nickerson, 1940), but it is less important when examining near-white substances which have no strong absorption bands. It is not generally possible to employ a coloured illuminant to exaggerate small departures from white in the way that one would, for example, employ a blue-green or a monochromatic yellow to exaggerate differences between similar red substances. The quantitative effect of change of illuminant on a white substance or a match between two near-white substances can be calculated exactly, the calculation requiring less precise data than when strongly coloured substances are considered. For most purposes the best illuminant for examining white substances will be the equi-energy source (C.I.E. 1931), which may be obtained fairly closely from a 2848° K. light source in the same way as the C.I.E. illuminants B and C by 1 cm. thicknesses of each of the following Davies-Gibson filters (Davies, 1934):—

Solution E_1

Copper sulphate ($CuSO_4\ 5H_2O$)	2·954 g.
Mannite ($C_6H_8(OH)_6$)	2·954 g.
Pyridine (C_5H_5N)	30·0 ml.
Water (distilled) to make	1000·0 ml.

Solution E_2

Cobalt ammonium sulphate ($CoSO_4(NH_4)_2\ SO_4\ 6H_2O$)	28·440 g.
Copper sulphate ($CuSO_4\ 5H_2O$)	17·840 g.
Sulphuric acid (s.g. 1·835)	10·0 ml.
Water (distilled) to make	1000·0 ml.

The total transmission factor of the combined filter is 19% and the nearest Planckian colour temperature is 5420° K.

Visual tests should always be made with a specimen of ample size, preferably subtending more than 10° at the eye, and with ample illumination, preferably over 10 f.c. If the surface of the sample has a definite texture, the standard method of illumination at 45° and observation at 90° should be adopted.

Visual measurement of the colour of a white light follows the same lines as for a white substance, as it is general practice to illuminate a standard white surface by the light under consideration. Freshly smoked magnesium oxide provides a suitable standard surface, although care is needed in its preparation (Gibson, 1938). Photoelectric measurement does not usually require the intermediary of a standard white surface, as the sensitive element is illuminated directly by the white light or by the light reflected from the surface under examination.

§ 4. INSTRUMENTAL METHODS FOR WHITE SUBSTANCES

The straightforward method of comparing two materials side by side is surprisingly critical, and quantitative methods may be hard taxed to put a figure on differences which are quite obvious. This comparison is, however, generally too simple, and full allowance is not made for such uncontrolled variables as the intensity or the colour of the illumination, differences in texture or differences in background. One of the best means of comparison is the Lummer-Brodhun photometer head, in which the contrast field provides a very sensitive indication of colour difference when any brightness difference has been compensated. Simplified apparatus can be constructed with colourless glass mirrors or a large biprism, by which two similarly illuminated samples can be brought into optical juxtaposition. In all these comparative methods the effect of different illuminants can be closely reproduced by holding suitable colour filters over the eye.

The simplest measurement is that of reflection factor. This may be done visually by comparison with a standard white of known reflection factor, such as magnesium oxide, varying the relative illumination on the two specimens until a match is obtained. For accurate work a photometer bench is desirable, but there are instruments, such as the Hilger photometric comparator, which constitute a more convenient and compact apparatus and provide sufficient accuracy for most purposes. There are many proprietary photoelectric instruments, of which the Morganite reflectometer (Heys-Hallett, 1941) is typical, which give good results, provided the difference between the response curve of the cell and the standard visibility function is not large.

Visual measurement of colour is not easy and may be tackled in three ways:

(*a*) Direct colorimetric measurement.

(*b*) Measurement of reflection factor in several wavelength bands along the spectrum.

(*c*) Spectrophotometric measurement.

In method (*a*) the choice of the colorimeter is important as, for example, a 3-primary additive colorimeter will have such a discontinuous spectral energy distribution that the chromatic aberration of the eye is liable to reduce the accuracy of measurement, particularly with the small field size generally obtainable. Departures from white are usually due to unbalanced selective absorption, which can often be reproduced by subtractive colorimetry as in the Lovibond colorimeter, preferably employing the Schofield system (Schofield, 1939).

Method (*b*) has been considered mostly for assessing the colour-rendering properties of artificial daylight sources, and many papers have been published (Bouma, 1939) on the relative values of different wavelength bands. A British Standard method has recently been described (BSS. 950—1941) and may well be used for estimating departures from white. The apparatus required is the same as for reflection-factor measurement, together with a series of about 8 colour filters isolating different spectral regions. The results may be shown as a graph of reflection factor against the dominant wavelength of each colour filter, and thus much more information is given as to the origin of the departure from " white " than would be given by method (*a*).

Method (*c*) is a more precise form of (*b*), and its main advantage for near-white colours is that it provides a greater number of observations. Visual spectrophotometry is laborious and is generally treated as a last resort in the measurement of whites.

Visual measurements may be simplified by means of a colour amplifier (Pfund, 1920), in which multiple reflection is employed to increase the apparent selective absorption. The amount of departure from white can be magnified up to about ten times, and the gain in accuracy of measurement of a near-white is more than sufficient to compensate the slight doubt regarding the exact power of the magnification.

Photoelectric measurement of colour is still in the early stages of its development, and the three visual methods just described all have their counterparts in photoelectric instruments. Direct-reading colorimetry has been described by Winch (1937), and photoelectric reflection meters can be adapted for the same purpose (Perry, 1938), although these instruments are scarcely sufficiently accurate for near-whites. There are several photoelectric instruments for measuring reflection factor in different wavebands, most of which are based on the reflection meters mentioned above, and precision instruments for this purpose have been described by van Alphen (1939) and by Winch (1940). These instruments are well suited to measurement of whites and give as high an accuracy as the best visual methods. The instrumental readings are best recorded on a diagram of reflection factor plotted against dominant wavelength, but these may be reduced to a simpler description of the sample by calculation of the approximate C.I.E. trichromatic coefficients and of the integrated reflection factor. Photoelectric spectrophotometry has not developed in this country to

the same extent as in the United States, where the General Electric recording spectrophotometer (Hardy, 1938, and Wright, 1940) has provided the best method of industrial colour measurement up to the present.

Photoelectric indicating spectrophotometers have been constructed in several laboratories in this country, but their advantages over visual instruments have not been sufficiently great for them to be generally adopted. It is to be hoped that automatic recording spectrophotometry will become better known and used in this country, as this method is probably even better suited to the measurement of whites than of other coloured substances.

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(For Discussion see p. 104.)

THE MEASUREMENT OF NEAR-WHITES IN THE PAPER INDUSTRY *

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MS. received 6 *November* 1941

§ 1. INTRODUCTION

One of the most important properties of a sheet of paper from both maker's and user's point of view is its colour. Papers of all colours are in use, but of these by far the most important group is the near-whites, on account of their extensive use in printing.

There will be no need to emphasize the necessity for controlling, and hence measuring, the colours of papers. Customers' samples must be matched; and if a papermaker supplies paper according to contract over a period of time, it is essential that the colour of the product should be maintained within fine

* A paper read to the Colour Group on 25 September 1941.

limits. It looks bad if papers from different batches, nominally the same but differing slightly in colour, are bound up in a book together.

It would be difficult and unprofitable to attempt to set up a rigid definition of a " near-white "; most, however, of the near-white papers in common use are yellows of hue (dominant wavelength) 5500–6000 A., about 3–15% saturation (excitation purity), and 60–95% brightness (visual efficiency). A typical curve for a pre-war newsprint is shown in figure 1. The paper is made largely from mechanical wood and contains little or no added dye or pigment. Figure 2 shows a spectro-photometric curve for a paper which has had a small quantity of a blue-green dye added to the stock in order partly to neutralize its yellowish colour.

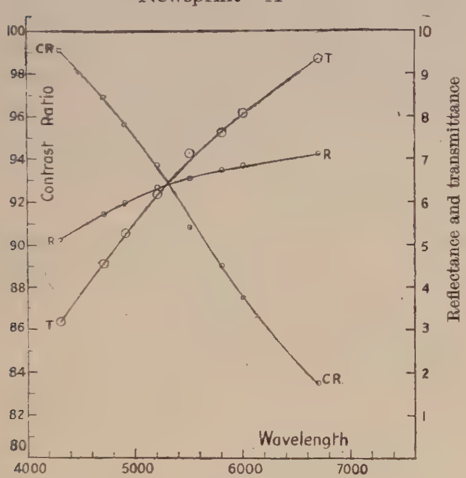

Figure 1. Curves for a typical undyed newsprint. 4000 to 7000 A.
RR=reflectance;
TT=transmittance (arbitrary units);
CR.CR=contrast ratio (a measure of opacity);
all plotted against wavelength of the incident light.

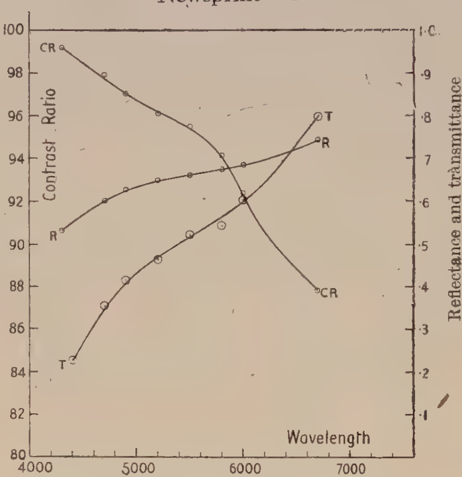

Figure 2. Curves for a newsprint containing a small amount of blue-green dye. 4000 to 7000 A.
RR=reflectance;
TT=transmittance;
CR.CR=contrast ratio;
all plotted against wavelength.

True neutral whites or greys are rare in paper. The papermaker accordingly adopts subconsciously a *natural paper white* (that is, the colour of untreated pulp or paper) as a working standard: papers of lower saturation than this he calls *blue-whites*, and papers of greater saturation he calls *cream-whites*; though all are, in reality, desaturated yellows. The standard of *natural white paper* almost certainly varies from one maker to another, and even with the same maker it probably varies from day to day. In any case, he would have difficulty in defining it or providing a typical example (Judd, 1935 and 1936). The terms blue-white and cream-white are thus more relative than absolute in the paper trade.

The papermaker and paper-user do not, in general, fully realize the threefold nature of colour (even for near-whites), and often insist on a single measure of brightness or whiteness (usually as a percentage) to define the colour of their products. This has led to confusion, and the paper trade's conception of brightness and whiteness needs careful examination, since it differs in important respects from the usage in other trades and, in particular, from the scientific usage of the term *brightness*.

§ 2. BRIGHTNESS AND WHITENESS

The term *brightness* has many possible meanings. In the paper trade it seems to be used to denote the relative quantity of light (independently of its colour) emitted from one surface as compared with another, estimated visually *without the aid of an instrument*. This usage (which for want of a better term I shall call visual brightness) is vague and hardly allows of quantitative specification. One can, however, usually grade papers in order of visual brightness and give a rough indication of the relative magnitudes of the visual-brightness differences. The order of papers graded in this way does not, in general, agree with their order as determined by reflectometer measurements.

Whiteness is one of a number of terms, usually terminating in -ness or -ability, to which it is difficult to give a satisfactory definition, although a certain vague meaning is easily apparent. *Whiteness* seems to be used in at least three distinct senses.

A. To some, *whiteness* is almost, if not entirely, synonymous with " visual brightness ", the " whiter " paper being the one which *appears* to reflect more light.

B. The term *whiteness* may also be used to denote the extent to which the colour of a paper approaches neutrality, regardless of the amount of light reflected from the surface. In other words, whiteness may be regarded as an inverse measure of saturation.

C. A perfect white surface is one which reflects *all* the light which falls upon it without selective absorption. *Whiteness* may therefore be taken to mean the extent to which a colour approaches that of perfect white, and such a definition would take into account not only the colorimetric brightness of the colour under consideration, but also the extent of its departure from neutrality. In this connexion it is not easy to give any quantitative measure of the extent of departure of a colour from perfect white. However, colours are defined by three independent variables and hence may be represented by points in a three-dimensional diagram. The points corresponding to all real colours fall within, or on the surface of, a solid, the apex of which represents perfect white. Colours that most nearly approach white are represented by points nearest the apex of the solid figure; while black is represented by the base of the solid. The most logical measure of the whiteness of a colour would involve the length of the line joining the point representing the colour to the apex of the figure; this length

could be found by calculation, but it would depend upon the system of co-ordinates adopted for plotting the figure. In any case, no single number will define a colour (if we exclude quite arbitrary systems), and it will always be possible to find a large number of distinguishable colours all of which have the same degree of whiteness, however this term be defined. For these reasons I suggest that it would be better to abandon the use of the word whiteness altogether in scientific and technical publications.

§ 3. "VISUAL BRIGHTNESS"

To return now to the subject of visual brightness, the published trade literature thereon is mostly unsatisfactory, a fact due probably to the vagueness of meaning of the term and to the mistaken idea that a single figure will suffice for grading a paper. Many " brightness meters " have been described; most of them are means for determining the reflectance of a sample in white or coloured light under more or less arbitrary conditions, and it is usually agreed that a reflectance measurement made in *blue* light gives results in closest agreement with visual judgment (Davis, 1935; Hunter, 1937; Farebrother, 1937).

On this basis depends the General Electric Reflection Meter (Davis, 1935), which is an instrument for determining the reflectance of a specimen when illuminated at 45° and viewed normally in light of wavelength 4500 A. This reflectance is often known in America as the brightness of the paper. Very careful calibration of the instrument is necessary, as the results depend upon the spectral sensitivity of the photocell used, and several other factors. The instrument has been found useful for comparing closely similar batches of paper, but otherwise it is of limited value. M. N. Davies (1935) points out that this use of the term " brightness " " appears to be peculiar to the paper industry. It is probably closely approximated by, if not synonymous with, the term whiteness used with reference to white textiles. It appears to be much more closely associated with the quality purity than with the quality brightness as the latter two terms are used in spectrophotometric colorimetry."

This apparently general preference for papers of lower saturation, even if this entails some loss of colorimetric brightness, is remarkable. At first I thought it was probably due to unconscious bias on the part of trained paper graders, who may prefer highly-dyed, nearly neutral papers to yellower papers as being of better " quality ", and therefore, perhaps, by association, of greater brightness. I therefore carried out some simple experiments on 20 members of the staff of Patra (the Printing and Allied Trades Research Association). Most of these observers had had no previous experience in grading papers and were unacquainted with the literature of the subject. The question of bias may thus be eliminated.

The observers were asked to choose the brighter from pairs of sheets placed in turn before them. Eight sheets were used, giving a total of twenty-eight pairs, all of which were considered independently. The sheets were placed on a table in front of the observer and illuminated by diffuse light coming from a

window on the left. No other observers were present during each experiment and no comparison of results was made until all opinions had been recorded.

A full description of the results has already been given in Patra Research Report No. 2, and I shall give only a summary of the main points. Figure 3 shows the approximate spectro-photometric curves of two papers marked for reference " 8991 " and " Bond ". It will be seen that 8991 has the lower reflectance throughout the visible spectrum, but the curve of Bond has the steeper slope. Colorimetrically, Bond is the brighter paper; it also has the greater saturation—that is, it is yellower in colour than 8991. Yet of the twenty observers, eighteen expressed a decided preference for 8991 as the brighter,

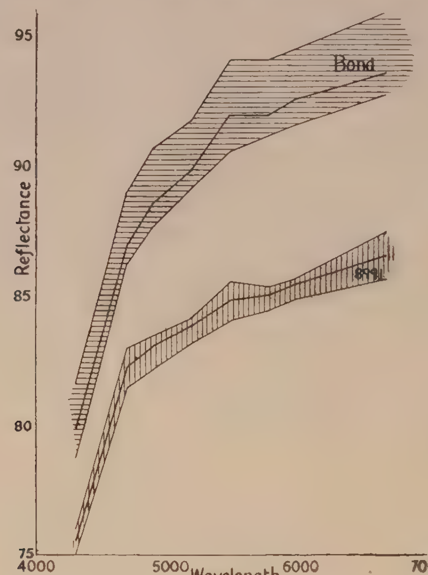

Figure 3. Approximate spectrophotometric curves of two papers. 4000 to 7000 A. The widths of the shaded bands indicate the variations in reflectance observed over the surface of one and the same sheet: the continuous lines near the middle of each band represent the mean values. 18 observers out of 20 considered 8991 to be the *brighter*.

while two were uncertain. No one voted for Bond as the brighter. Results with the other pairs left no doubt that the principal factor influencing judgment was the slope of the spectrophotometric curve (that is, the saturation of the sample, rather than its colorimetric brightness.) In the paper mill it is common practice for the beaterman to add a little blue dye to the stock in order to " brighten " the paper. From the physicist's point of view, this is nonsense; the dye can only help to neutralize the natural yellowish colour of the pulp, and in doing so it must inevitably reduce the reflection factor of the finished paper: however, these experiments show that, as far as visual judgment goes, the beaterman is behaving quite correctly.

From a study of the results, it was possible to arrange the eight test papers

in order of their " visual brightness ". Reflectance measurements were also made on them for light of different wavelengths. Table 1 shows a summary of

Table 1

Order of sheets according to			
Visual brightness	Reflectance at		
	6700 A.	5500 A.	4300 A.
1	Bond	Bond 8992	8974
8974	8992	8974	1
9138	8974 1	1	8992
8292	8292	8292	8292
8992 9089	9138	9138	Bond
8991	9089	9089	9138
Bond	8991	8991	9089
—	—	—	8991

the results. In the first column are the papers arranged in order of " visual brightness ", starting with the brightest at the top; in the second, third, and fourth columns the papers are arranged according to reflectance measurements made at wavelengths of 6700, 5500 and 4300 A. respectively. It will be seen that reflectance measurements made in red and green light provide a very poor measure of the visual grading of samples. In blue light the agreement is better, though it is still far from good: this gives some experimental justification for the use of the G. E. Reflection Meter already mentioned.

In table 2, I have compared the orders of the sheets according to " visual brightness " and also according to $2B-R$ and $R-B$, where R and B are the

Table 2

Order of sheets according to		
Visual brightness	$2B-R$	$B-R$
1	8974	8974
8974	1	1
9138	8992	8292
8292	8292	8992 9089
8992 9089	9089	9138
8991	9138	8991
Bond	Bond	Bond
—	8991	—

reflectances in red and in blue light respectively. It will be seen that the agreement is now much better, particularly with the $R-B$ formula. (The $2B-R$ formula has been given by Samuelsen, 1937.) It is clear, therefore, that what I have

called *visual brightness* is closely related to the *saturation* of the colour, and *not* to the quantity of light reflected; indeed, within limits, the colorimetric brightness seems to be of comparatively small importance in affecting estimates of visual brightness. This result, of course, is valid only for near-white colours.

It thus appears, from these experiments, that the eye is, for near-whites, more sensitive to small changes in saturation than to small changes in colorimetric brightness. The effect of a small increase in saturation is, to most observers, to produce a decrease in visual brightness. As the saturation is increased, the colour obviously becomes yellower to all observers: the interesting fact, however, is that the initial increase in saturation, if not too great, makes itself apparent rather as a decrease in *brightness* (visual) than as an increased yellowness. This may be illustrated by the sheet marked Bond, which almost all observers agreed was dull in comparison with the others. Several, however, failed to notice any particular yellow or cream tint about it, but described it as comparatively grey.

It is apparent from the foregoing that single brightness, whiteness or reflectometer measurements are only of very limited value in specifying the colours of near-white papers. Near-whites are still colours, and as such they can only be adequately defined in terms of the C.I.E. system. The accuracy required and the type of instruments most suited for the purpose must therefore next be considered.

§ 4. METHODS OF MEASURING OR DEFINING THE COLOUR OF PAPER

A. The making of colour matches with samples in a colour catalogue. The obvious objections to such a system of colour notation are: (1) the selection of the standard colours for the catalogue must be arbitrary; (2) it is difficult to reproduce the standard colours accurately; (3) it is left to the individual judgment of the observer as to which of the standard colours is the nearest match to the sample, and opinions here can vary to a surprising degree; and (4) it is difficult to ensure permanency of the standard colours, and their fading, when it occurs, cannot easily be detected.

B. Matching the colour of the specimen with that of a combination of filters chosen from a set of various " depths " of three primary colours. The Lovibond tintometer is of this type.

C. The visual trichromatic colorimeter, such as the Guild and Donaldson colorimeters.

D. The photoelectric trichromatic colorimeter. Several of these have been designed and constructed. They vary from simple reflectometers of doubtful value, making use of arbitrary red, green and blue filters in front of the photocell, to elaborate precision instruments, of which the Blancometer is probably the best-known example.

E. The monochromatic colorimeter, in which the colour of the sample is matched visually with a mixture of white light and one of the colours of the

spectrum. This instrument is used mainly for research on colour vision, and I am not aware of any cases in which it is in use as an industrial colorimeter.

F. The "abridged" spectrophotometer, or Reflectometer, which makes use of a set of filters for changing the wavelength of the incident light.

G. True spectrophotometers, in which the sample is illuminated by monochromatic light.

I do not propose to give further consideration to methods A and E. Whether the other methods can be considered suitable or not depends mainly on what degree of tolerance in colour measurements is permissible in commercial work.

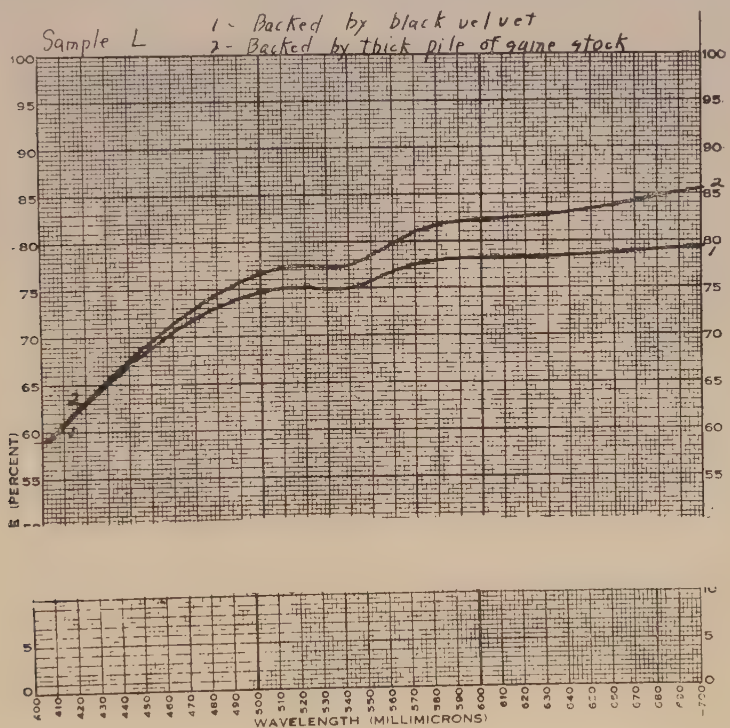

Figure 4.

There is little published information on what degree of accuracy papermakers require. It is, however, probably related to the least perceptible difference in chromaticity obtainable under the best conditions of observation; and in this connexion it must be remembered that the sensitivity of the eye when one is observing large sheets side by side in a good light is not necessarily the same as its sensitivity when one is working with the poor light and narrow 2° field of a colorimeter. Judd (1936) gives a tolerance figure of 0·0005 in the trichromatic units. This is very small.

In order to gain information as to the degree of sensitivity required for commercial work, six batches of paper were obtained by Patra from a papermaker.

These were taken from a single run in the mill and were identical except for small colour variations. These variations were so large as to make it necessary to sort the paper into six distinct batches, so that the colour differences clearly fell outside the commercial limits of tolerance. Samples of these papers and data for them have been circulated in a Patra Research Report (No. 3). By courtesy of the Research Laboratories of Interchemical Corporation, New York, spectrophotometric curves were taken for these papers on the Hardy recording spectrophotometer. A typical curve is shown in figure 4. From these curves the C.I.E. trichromatic units and brightness factors were calculated for Illuminant B by the thirty-selected-ordinate method: the results are summarized in table 3.

Table 3

Illuminant "B". Self Backing

Paper	Tristimulus values.			Trichromatic coefficients	Brightness (Y)
	$\bar{X}$	$\bar{Y}$	$\bar{Z}$		%
C	78·52	78·84	58·00	$0{\cdot}3646X+0{\cdot}3661Y+0{\cdot}2693Z$	78·8
CP	76·66	76·93	56·97	$0{\cdot}3641X+0{\cdot}3654Y+0{\cdot}2706Z$	76·9
L	79·24	79·69	59·99	$0{\cdot}3620X+0{\cdot}3640Y+0{\cdot}2740Z$	79·7
BB	79·63	80·05	60·50	$0{\cdot}3617X+0{\cdot}3636Y+0{\cdot}2747Z$	80·0
BC	78·53	78·77	57·26	$0{\cdot}3660X+0{\cdot}3671Y+0{\cdot}2669Z$	78·8
PR	76·12	76·32	56·03	$0{\cdot}3651X+0{\cdot}3661Y+0{\cdot}2688Z$	76·3
D	76·55	76·83	56·89	$0{\cdot}3640X+0{\cdot}3654Y+0{\cdot}2706Z$	76·8

The papers marked CP and D were identical, but were sent for test as different batches in order to test the performance of the instrument. I will not vouch for the absolute accuracy of the figures (this depends upon the calibration of the instrument, which, of course, we had to take on trust), but I do consider that they give a fair measure of the sort of colour differences that we are called upon to detect. It will be seen that for the extreme papers of the range the differences in the coefficients of X and Y are only about 0·004, and the extreme brightness differences are only about 4 %. Thus to distinguish between the papers with certainty involves measurements to a considerably higher degree of precision than this; indeed, from the figures it appears probable that it will be necessary to estimate the fourth figure of the trichromatic units and the brightness factor to 0·2–0·3 %. This is well outside the limits of visual colorimetry.

As a check on these results, we attempted to measure the colours of these papers on a Donaldson colorimeter: we found it quite impossible to separate the papers with any degree of certainty. A member firm also made measurements for us with a Lovibond tintometer, and reached similar conclusions. On the other hand, a laboratory reflectometer fitted with a series of eight "monochromatic" filters showed clear differences between the papers, and also yielded

curves in fair agreement with those given by the Hardy instrument (figure 5); unfortunately, however, these curves were partly characteristic of the instrument as well as of the paper, and consequently could not be made to give results on an absolute basis.

From these results we have concluded that visual colorimeters are unsuitable for the measurement of near-whites in the paper industry. Personal errors are much too large and they are insufficiently sensitive. I believe it is a fact that

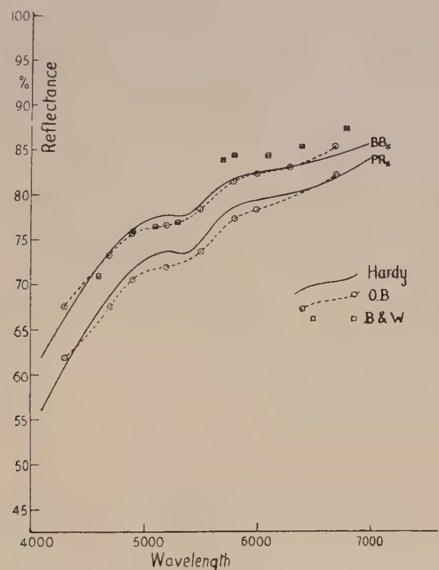

Figure 5. "Hardy"-spectrophotometric curves given by Hardy recording spectrophotometer. 4000 to 7000 A.
"O.B." and "B & W": points given by two "abridged" spectrophotometers.

the eye is more sensitive to small differences of chromaticity when the observer is free to compare large samples side by side in a good light than it is when vision is restricted to the weak light and narrow field of a colorimeter. Thus the failure of these instruments is due not to any fault in their design or construction, but to the characteristics of the human eye.

The choice of instruments is thus restricted to photoelectric colorimeters or spectrophotometers. Each of these classes of instrument has its own particular advantages and disadvantages.

§ 5. REQUIREMENTS OF AN INSTRUMENT FOR INDUSTRIAL PURPOSES

In the ideal case it should be possible to specify the colour of a piece of paper in terms of the C.I.E. system, and to reproduce that colour within commercial tolerance limits in other works without the need for an exchange of samples. If one orders a shaft to be turned to a certain length and diameter one can be certain, provided the work has been properly carried out, that the shaft will fit

the job for which it is intended; there is no need for one to send a sample shaft for the engineer to match. A similar state of affairs should be possible with colours. However, this involves measuring the trichromatic coefficients and brightness factors to a very high degree of absolute accuracy, so high, indeed, that special difficulties are introduced. One may therefore decide to abandon hopes of getting the requisite degree of absolute accuracy, and to rest content with producing an instrument giving more or less arbitrary readings for internal use in the works only.

If the second, less ambitious, course is followed, the main requirements are: (1) adequate sensitivity, (2) stability, (3) the results, even if arbitrary, should be clearly related to what it is desired to measure. The second requirement is necessary because if one makes measurements on the same paper over a period of time and variations are noticed, one does not want to have to analyse the results to find out whether the variations are due to changes in the paper, or to the erratic behaviour of the instrument. The third requirement is necessary because it is easy to construct so-called colorimeters which, although they are sensitive and give repeatable readings, are liable to give misleading results; for example, a badly designed colorimeter may give identical readings for two papers which differ perceptibly in colour.

§6. SPECTROPHOTOMETER OR PHOTOELECTRIC COLORIMETER

If one is satisfied with an instrument intended for comparative results for use in the works only, fairly satisfactory approximate spectrophotometers and colorimeters can be constructed or obtained without much difficulty.

The type of "abridged" spectrophotometer in common use is essentially a photoelectric reflectometer equipped with a number (seven or eight is convenient) of narrow-band filters. With such instruments, sensitivity, coupled with a reasonable degree of stability, can easily be obtained, and they are not costly. They are particularly useful if the effect of adding dyes to a paper stock is being studied. On the other hand, the readings cannot be converted into absolute (C.I.E.) units, and they retain an arbitrary character due to instrumental factors that cannot be completely eliminated from the results. These instrumental errors rapidly become greater as the colour to be measured departs from white.

The term *photoelectric colorimeter* covers a wide range of instruments, from crude reflectometers making use of arbitrary red, green and blue filters to highly-developed instruments of the "Blancometer" type. Most of the cheaper type are decidedly unsatisfactory; they are insufficiently sensitive, and the readings, if not quite arbitrary, contain large instrumental factors for which it is difficult to make proper correction. A few of the better types, however, can be very useful for routine control purposes. In them the instrumental errors have been reduced to comparatively small dimensions, and their sensitivity and stability are adequate. They have the advantage that they furnish approximate readings in terms of C.I.E. units *directly*. On the other hand, the better instruments are

costly, they give results for one type of illuminant only (unless specially constructed to carry additional filters for each extra illuminant required), and for some work a mere statement of colour in terms of trichromatic units gives much less information than the complete spectrophotometric curve. The C.I.E. units can always be calculated from the curve (though the calculation is tedious), but the converse does not apply.

If, however, we are aiming at absolute accuracy within the limits of commercial tolerance, the problem is much more difficult. Only the best instruments need be taken into consideration, and their cost will inevitably be high. The Blancometer may be capable of giving the necessary accuracy provided it is very carefully and frequently calibrated. The " abridged " spectrophotometer here is useless. Since it is thus necessary to employ an expensive and probably delicate instrument, it is most economical to adopt the instrument which gives the fullest possible information about the colour to be measured. This instrument is undoubtedly the spectrophotometer. A recording spectrophotometer, such as Hardy's,* has obvious advantages, but it is not absolutely essential.

I can see little possibility of the general adoption of recording spectrophotometers by private firms. For one reason, to all but the largest firms the cost of the installation would probably be considered prohibitive; for another, unless the spectrophotometers were carefully and intelligently used, a state of chaos would almost certainly arise and the use of instruments probably abandoned altogether.

There is, I think, a way out of the difficulty. Most papermakers can manage very well provided they have working standards which they have to match. This can be done by eye or with the help of one of the " abridged " spectrophotometers or photoelectric colorimeters already discussed. The trouble is that paper is always liable to discolour with age. Slight discoloration in an alleged standard is difficult to detect, and unless noticed and corrected, a mill may easily find itself producing progressively yellower and yellower papers. Moreover, there are at present no accepted standards to which to work: the pre-war procedure used to be for a customer to send in a sample of paper to the mill and expect the mill to match it exactly, regardless of whether the mill had a close, but not exact, match already in production as a stock line.

Most printing could be done without disadvantage on one or other of a limited number of standard papers. If the colours of these standard papers could be agreed upon, a very useful simplification would result. The next step, in my view, would be for a Paper Testing Centre to be set up, which would undertake to provide spectrophotometric and colorimetric data for paper samples, and to assist in their interpretation in cases of difficulty. The mill would then send its standard lines along regularly for test, say once a month. In this way any

* An account of the design, construction, calibration and operation of the Hardy recording spectrophotometer will be found in a series of papers in the *J. Opt. Soc. Amer.* 1938, **28** (10), 360-386.

alterations in the colours of the samples would be detected before they had time to become serious, and if necessary, new standards could be prepared. Once the mill was satisfied that the standards were satisfactory, the production of papers to match the colour standards would be found straightforward. Papers of other colours would then only be made to special order.

It is, I believe, only along the lines of standardization and co-operative testing that the present difficulties and wastage of materials involved in the matching of customers' samples can be overcome.

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(For Discussion see p. 104.)

THE WHITENESS OF CINEMA SCREENS *

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MS. received 6 *November* 1941

§ 1. INTRODUCTION

When black and white films are projected, the apparent colour of the screen is of no practical importance; indeed, some producers use either lavender or sepia stock on occasions in order to enhance certain dramatic effects. The advent of colour changed all this, and the whiteness of the screen is now of first-class importance if colour distortion is to be avoided. An immense amount of work has been expended on the development of colour processes such as Technicolor and Dufaycolor, but, although release prints of really high quality are now available, little has been done to provide undistorted projection. A start has, however, been made on a survey of projection conditions in a large number of cinemas.

The special instrument, which was developed for this work, consists of a lens which throws an image of the screen on to a photo-electric cell connected to a galvanometer which thus records the total amount of light received from the screen. Arrangements are made for the insertion of suitable masks to enable

* A paper read to the Colour Group on 25 September 1941.

part only of the screen to be examined, and also for the insertion of a tri-colour set of filters to enable measurements of colour to be made.

The instrument was primarily designed for use by service engineers when testing and adjusting the projection equipment, and is therefore invariably used

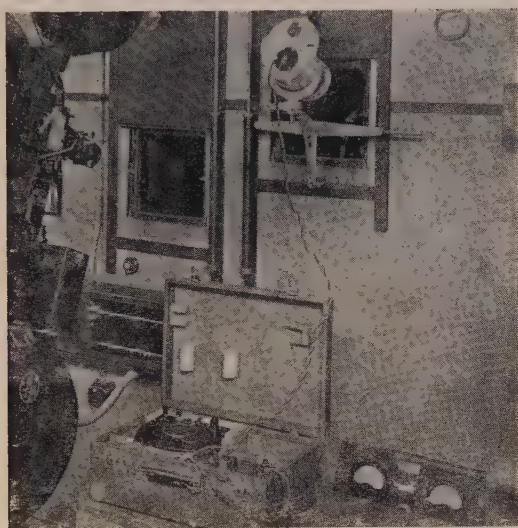

Figure 1. The reflectometer mounted in the projection room.

from the projection room (see figure 1) with shutter running but no film in the gate.

§ 2. MEASUREMENT OF BRIGHTNESS

As the image is smaller than the active surface of the photo-electric cell, it is necessary to correct the readings by means of a chart to obtain a measure of brightness. Before the development of this instrument, it was customary to measure the incident light on the screen, and most people connected with projection have some idea of the values to be expected, but their knowledge of photometrical units is generally insufficient to enable them to appreciate the difference between foot-candles and foot-lamberts, and endless confusion resulted. To overcome this, the readings taken in a large number of cinemas were examined and, after eliminating unusual and obsolete equipments, an average was obtained. This average was then used as a datum and all readings were expressed as a ratio; thus, if a cinema showed a reading of 66 scale divisions, this was divided by the datum reading of 60 and the cinema was then said to have a *specific brightness* of 1·1, indicating that it was 10% above the datum (which is, incidentally, approximately 5 foot-lamberts).

Table 1

Case	Class of house	Specific brightness	Remarks
31	Large West End	0·52	
16	Large West End	1·21 1·65	Change due to re-treatment of screen
95	Large West End	0·80	
91	Modern large town	0·76	Modern arcs under-loaded
89	Modern large town	0·82	Same box equipment as 91, but fully loaded
74	Small country town	0·78	A.C. high-intensity
75	Small country town	0·28	Low-intensity
93	Suburb	0·16	
86	West End News Theatre	8·60	Same equipment as 89
56	West End News Theatre	4·43	Beaded screen
87	Renter's Theatre	0·314	Low-intensity arcs
96	Renter's Theatre	6·65	Obsolete arcs
36	Studio Rush Room	4·87	
14	Two Rush Rooms in Studio	1·67 2·61	
37	Colour Film Laboratory	2·62 1·15	As found After adjustment with Reflectometer
A.12		0·24	Dirty screen, modern lamps
A.16	Kinemas in Sydney and Melbourne	0·98	
A.19	Kinemas in Sydney and Melbourne	2·0	
A.60	Kinemas in Sydney and Melbourne	0·59	A.C. high-intensity

The above table shows the magnitude of the variation in brightness which is encountered in practice.

§ 3. COLOUR MEASUREMENT

Two methods of expressing results are in use. Prediction of visual effect can obviously only be based on comparison of the light under review with a standard. We were, therefore, forced to adopt a standard at the commencement of our work, and the standard selected might be described as an average of high-intensity projection arcs.

(a) *Equal-green method*

This method of comparing two colours depends on eliminating one of the filter readings, always the green, by expressing the set of readings as a percentage of the green reading. Even then the set of readings for a kinema would be quite meaningless to everyone, and recourse is therefore had to the solution worked out for screen brightness. That is, all readings are expressed as a function of the datum and termed *specific colour*.

The example given in table 2 shows readings of a kinema which has a light which is 31 % high in red and 25 % deficient in blue. It is possible with a little practice to get some idea of what the visual effect will be, but as two specific colours are involved it is not easy. This example is one of the simpler ones,

but sometimes both blue and red specific colours exceed unity. A method of reducing the description to a single expression has therefore been evolved.

Table 2. Specific colour

Colour	Actual readings	Expressed as % of green	Standard colour	Specific colour
Red	6·3	59	45	1·31
Green	10·7	100	100	1·0
Blue	4·0	37·5	50	0·75

(b) *Dominant hue*

Knowing the specific colours, calculate the percentage of the total represented by each, thus:—

$$\begin{array}{ll} P_r & 1{\cdot}31 \\ P_g & 1{\cdot}0 \\ P_b & 0{\cdot}75 \\ \hline & 3{\cdot}06 \end{array}$$

therefore

$$\% \, R = \frac{1{\cdot}31 \times 100}{3{\cdot}06} = 42{\cdot}8$$

$$\% \, G = 32{\cdot}7$$

$$\% \, B = 24{\cdot}5$$

Similar treatment of the standard colour (P_r 1·0, P_g 1·0, P_b 1·0) obviously gives $33\frac{1}{3}$ for each.

These colours are then plotted on the wheel diagram (figure 2). Standard colour is represented by the point at the centre of the diagram, since if we plot $33\frac{1}{3}$ units towards green, turn 60° and plot $33\frac{1}{3}$ units towards blue, followed by $33\frac{1}{3}$ units in the direction of red, we return to the point from which we started. The other set is then plotted in a similar manner, and the triangle fails to close, indicating a colour difference of 15·6 % orange, the visual effect of which is easily pictured in the mind.

When using this diagram to compare two non-standard lights we proceed as follows:—

First, multiply each colour reading of one light by the requisite factor to bring it to $33\frac{1}{3}$ and thus make it the centre of the diagram.

Now multiply each colour reading of the other light by the same factor as was used for the same colour of the first light. Add up the three results, and multiply each by (100/total). The results thus obtained will add up to 100, and are the percentage readings required for the colour wheel. Note that in this case it is not necessary first to calculate the specific colours.

This method is not very accurate if the lights are far off white, but is sufficiently close for the purposes under review.

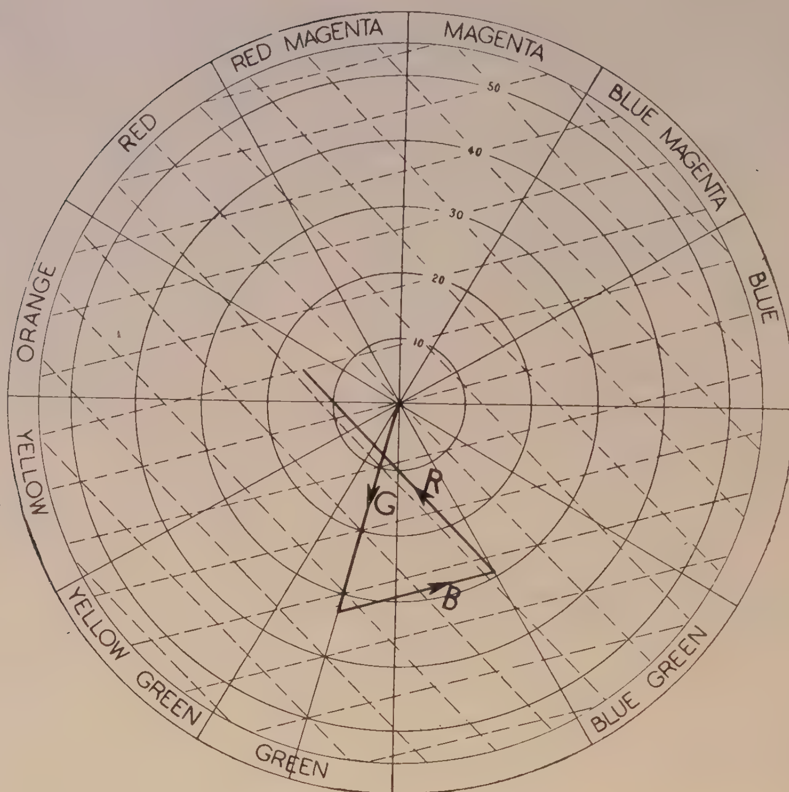

Figure 2. Method of plotting dominant hue on colour circle.
(Reproduced by the courtesy of the British Kinematograph Society.)

It is now possible to study the variation in colour encountered in kinemas, and this can best be done by considering the various links in the chain connecting the arc and the eye separately.

§ 4. VARIATION OF COLOUR

The colour of projection arcs varies for a number of reasons. The shaded area in figure 3 shows these variations for a representative modern high-intensity mirror arc designed to operate round about 50 ampères. The lamp was tested over a range of 10 ampères with several grades of carbon of different makes. The lamp was properly handled throughout the test, and the shaded area therefore represents the envelope which will be obtained when the lamp is run strictly in accordance with the maker's intentions. Cases of under- or over-loading will, however, occur in practice.

The difference between high intensity and low intensity is most marked. The point A in the diagram indicates the average of low-intensity mirror arcs when correctly run at approximately full rating. Reduced loading will, of course, make the light even redder.

The effect of unusual design is shown by point C, which indicates the difference between the Stelmar arc and the representative lamp represented by the shaded area, while B is the point for an A.C. high-intensity projection arc.

Some attempts have been made to use metal mirrors instead of glass, and point F, comparing a rhodium mirror with a glass one, is therefore of interest.

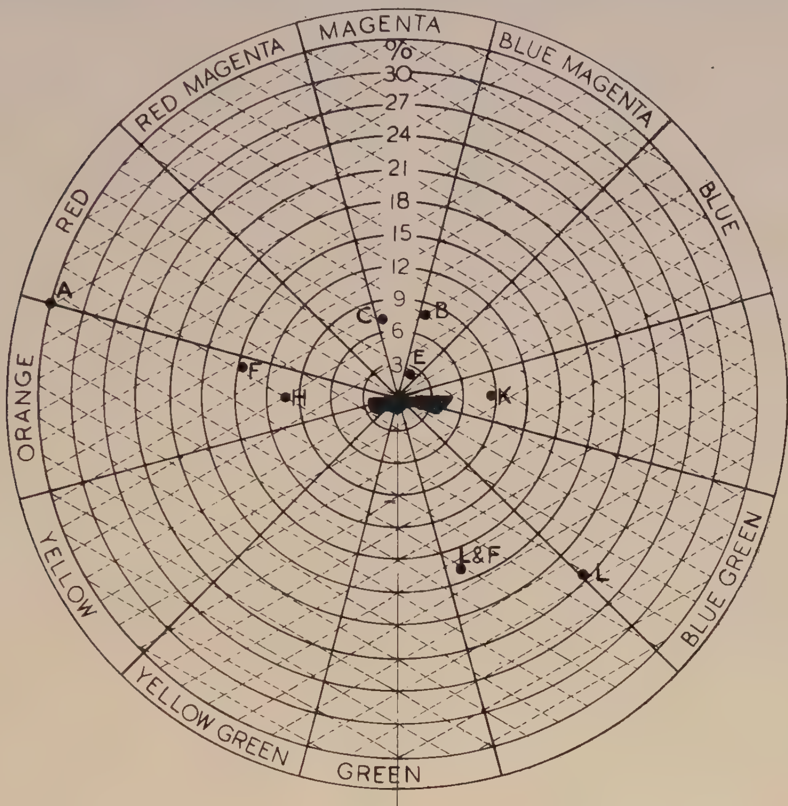

Figure 3. Examples of readings plotted on colour circle.
(Reproduced by the courtesy of the British Kinematograph Society.)

Screens have a powerful influence as illustrated by point H, which shows the change in colour during the life of the screen. Point K is for a beaded screen, while all other types of screen tested lie, when new, between 2·5 % orange and 3·5 % blue-green. Point E shows the change effected by re-treating the screen in a particular kinema.

§ 5. DISTORTION OF THE PROJECTED PICTURE

Screen brightness and the apparent colour of the screen each have their own distorting effect on colour films. Experiments on the effect of brightness have been carried out in the Morganite Laboratories, and it was found that with both black-and-white and colour films, the picture improved progressively as the brightness increased, that a definite maximum was reached with black and white

at a specific brightness of about 10, above which graininess became troublesome, but that even at a specific brightness of about 50 no limit could be found for colour films. The effect of low brightness was much more serious on colour than on black-and-white because more colour distortion resulted. At specific brightness below 1·0 all colours appear to be mixed with brown, thus giving the picture an "old master" appearance. Screen deterioration, therefore, may distort the apparent colour in two ways, firstly by loss of reflectivity and secondly by introducing an orange hue.

Attempts have been made to correct the apparent colour of the screen in various ways, but these all seem to be doomed to failure because they all reduce the screen brightness, and as nearly all screens are inadequately lit, the distortion introduced by this reduction usually exceeds the gain in truth due to correction of colour.

The use of blue glass mirrors is an example of the attempts which have been made. The object was to correct the typical orange hue of low-intensity arcs to the blue of the modern high-intensity. A particular mirror of this type was compared with a clear glass mirror and found to reflect 83 % through the blue tricolour filter, 60 % green and 49 % red.

Changing the clear glass mirror of a low-intensity arc for this mirror altered the dominant hue of the light incident on the screen from 21 % red to 8½ % orange, which is still far from perfect. This improvement was obtained at a cost of 39 % of the screen brightness, and as low-intensity arcs have, as their name implies, only about ⅓ of the light output of a high-intensity arc, this cost is prohibitive.

§ 6. CONCLUSION

Unfortunately, insufficient work has been done to enable the requirements of screen brightness and colour to be laid down. Release colour prints are now so good that, when projected under the conditions for which they are made, the colour reproduction is perfect within the discrimination of the average audience.

The deviations of dominant hue usually encountered are within the scope of the colour processes, and it may therefore be said that, as a release print must be shown in any cinema, the prime necessity is standardization, and exactly what is standardized is of secondary importance.

The problem of brightness is, however, not so easily settled, because the great majority of the cinemas in this country have inadequate brightness, and it is therefore difficult to see how the rendition of colour films can be improved without wholesale re-equipment.

DISCUSSION*

Mr. J. W. Perry. One is reminded by this sequence of papers of the dictum of Prof. Freundlich that "Science consists in approximations", which may here be understood in relation to the growth of colorimetry in both a qualitative

* On the three papers above.

and a quantitative sense. Recent applications of colorimetry have made progressively increasing demands upon accuracy as the conflicting effects of physical requirements and conditions of accuracy come to be more fully recognized with the growth of knowledge of their individual relationship to the discrimination limen. The earlier stages of this evolution may be traced through application of colour names and samples.

Whiteness in a physical sense may be regarded as a quantified colour name, and in the sense that colours may be said to " have whiteness ", this property would be possessed by the colours represented within a certain spatial region of a solid colour chart. Such a colour region may have its own local boundary, as would be required by the popular meaning of whiteness, or be extended to cover the whole colour diagram, if a meaning parallel to colorimetric desaturation be adopted. General agreement on this point and on corresponding graded requirements in regard to accuracy under various conditions is very desirable.

With reference to the Blancometer, it may be generally observed that as one may safely assume that the near-whites met with in practice are, almost without exception, of restricted selectivity approximately characteristic of their colour, the accuracy of colour measurement by such means, employing a colorimetrically corrected photocell-filter combination, must necessarily be sufficient for any given purpose within some definite colour region, and I am pleased to note Dr. Harrison's remarks in this connection from the practical standpoint. The work which is being done by him and others in investigating and evaluating the traditional generalizations and practical concepts employed in ancient arts and trades is commendable, and as unknown phenomena may lie obscurely hidden in the application of industrial processes, such work may also be fruitful by bringing these to light. Such is the case apparently with the curious phenomenon relating to subjective-brightness comparisons which he describes and which, if genuine, may be of associative origin. Regarding these tests, I should like to ask him whether any special illuminant was used and the possible effect investigated of variations of colour temperature upon the results of the comparisons; also whether the effects of gloss were eliminated.

If this phenomenon is purely subjective and allied to colour temperature, it may have applications in the projection of colour films within the relatively broad colour tolerances there possible, through the use of a filter or mirror which would suitably re-adjust the implied colour temperature of the object " imaged " on the screen, to produce an apparent brightness increase.

Mr. H. G. W. Harding. As regards the paper by Mr. Holmes, the relative merits of magnesium oxide and titanium oxide for a standard white have been mentioned by Mr. Holmes and Dr. Jolley. In most colour measurements it is necessary to have a standard white, the reproducibility of which is above suspicion. At the National Physical Laboratory we have used as a standard a polished silver plate on to which magnesium oxide has been deposited to a

thickness of about 2 mm. A few measurements that I made some years ago indicated that although the colour and brightness of such a deposit varied appreciably between thicknesses of 0·01 mm. and 1·0 mm., the changes from 1·5 mm. to 2·0 mm. were very small. Deposits thicker than these are difficult to obtain since they fall away from the silver surface.

Dr. Harrison, in his paper, mentions the suitability of the Hardy spectrophotometer that is available in America for the work in which he is concerned. There appear to be no instruments of this type available here, but there are non-automatic photo-electric instruments which give satisfactory results. The time taken for a spectral reflection curve defined by forty points using the instrument at the National Physical Laboratory is about an hour. An instrument of this type may be of use.

Mr. Applebee. I wish to point out that, in addition to the factor mentioned (namely, the "colour" of the white screen and the "colour" of the various light sources), the resultant reflected light from the maintained lighting (i.e. the lighting that has to remain alight during the showing of the picture) and also from the tobacco-smoke-charged picture beam should be considered. Both these cause stray light to be reflected on to the screen, and this stray light is again likely to vary in each building owing to the dominating colours of the decorative motive of the walls. It may therefore give to the screen an orange tinge, or, in another cinema, possibly painted in the light greens, a blue tinge; add to this the "nicotine" colouring of the smokers, together with the insistence of certain licensing authorities in some parts of the country on a higher intensity of secondary and maintained lighting, and you have a most complicated problem when it comes to standardization.

Dr. W. D. Wright. I should like to ask Mr. Heys Hallett how far the standardization of the colour of cinema screens in this country would be affected by any corresponding standardization abroad, especially in the United States. If different standards were adopted in the two countries would it be possible to secure adequate compensation by modifying the printing conditions of colour positives?

I should also like to ask Mr. Heys Hallett whether he considers it possible and worth while to express his colour measurements in terms of the standard C.I.E. system or whether he regards the cinema problem as so specialized that an arbitrary system of measurement is to be preferred?

Mr. R. Donaldson. One of the problems that Dr. Harrison has discussed is very accurate colour measurement over a small part of the colour field, viz., the yellowish-white region. The trichromatic colorimeter is designed to measure all colours, both saturated and unsaturated, and to do this it must have a small photometric field and saturated primaries. The colorimeter can, however, be easily adapted to cover a small portion of the colour field with increased accuracy.

Greater precision of setting can be obtained by using the large Lummer-Brodhun contrast field instead of the normal 2° field. With the large field the eye works under very sensitive conditions, and any colour difference visible to ordinary viewing becomes more apparent in the Lummer-Brodhun field. The personal error due to the colour-vision characteristics of the observer may be reduced in two ways. Pale unsaturated filters can be substituted for the usual deep red, green and blue filters supplied with the instrument, so that big differences in spectral-energy distribution between the two halves of the photometric field are avoided. Alternatively it may be sufficient to choose a comparison standard close to the colour being measured. If the magnesium oxide is too far away a piece of calibrated matt opal glass may be suitable as a standard.

AUTHORS' replies to discussion.

Mr. J. G. HOLMES. In reply to Dr. Jolley, it has been my experience that different consignments of titania may have different colours, ranging from white to pale yellow, which cannot be correlated with any traces of impurities. This difference is probably attributable to the technique of manufacture, and renders the oxide unsuitable as a general reference standard. Titania forms a good undercoating for magnesium oxide and gives an opaque white of high reflection factor with only a thin coating of magnesium oxide. A better absolute standard is undoubtedly given by the silver-plate method described by Dr. Harding.

In reply to Mr. Perry's comments on the noun " whiteness ", there is no fundamental difference between the whiteness and the redness or yellowness of a colour. I have recently been able to find the limits of the area of the colour chart within which coloured light sources are called white, red, yellow, etc., and small areas of this type have been used for several years as a definition of a colour in standard specifications. When considering coloured materials, however, whiteness has a different meaning from redness or yellowness, but the popular meaning of the word is so deeply established that it would be difficult to give it any quantitative significance which would be generally acceptable.

Dr. V. G. W. HARRISON. In reply to Mr. Donaldson, we have had our Donaldson colorimeter fitted with Lummer-Brodhun contrast field, which can be used alternatively to the normal 2° field. With the large field there is undoubtedly a considerable gain in sensitivity, but the reproducibility of readings for different observers and even for the same observer on different days remains disappointing. Mere sensitivity without stability and reproducibility is useless in industrial work. I have not tried in the colorimeter the desaturated filters which Mr. Donaldson mentions, but we have had some experiments made with the Lovibond tintometer, which makes use of very desaturated filters, although the colour mixture here is subtractive and not additive. The errors with the Lovibond instrument were of the same order as those observed with the Donaldson colorimeter. While I do not claim to have said the last word

on this subject, from the evidence available to me I consider that it is unlikely that visual colorimeters can be improved to such an extent that they will be able to satisfy the very exacting demands of commercial work.

In reply to Mr. Harding, the question whether a non-recording spectrophotometer is suitable for measuring the colours of papers depends on how many papers have to be measured during the day. For research purposes, where only two or three curves are required per day, the type of instrument in use at the National Physical Laboratory should be quite satisfactory; but for commercial work a non-recording instrument will be much too slow. It is of interest to note that Hardy developed his recording spectrophotometer precisely because he found available non-recording types much too slow for normal routine work.

As regards the question put by Mr. Perry, the tests on " visual brightness " were made in diffused daylight, direct sunlight being avoided. The experiments were carried out in the late morning and early afternoon of a single day, and there were no marked changes of weather during the period of the tests. I should not care to forecast what would be the effect of large changes in colour temperature on the results. The samples were laboratory sheets with a rough matt surface having no perceptible gloss.

I should like to point out that I started tests as an avowed sceptic and finished by " discovering " something which had already been known for many years in the paper trade; I have thus good reason to believe that the results are genuine.

Mr. Heys Hallett. The answer to Dr. Wright's first question is that projection illuminants are much the same in all countries, and it is therefore most improbable that a country would set up a standard that would cause any anxiety to the printers. It is probably desirable to avoid having to make special prints for each country, and an International Standard should therefore be aimed at. I believe that this country and the U.S.A. are the only ones having colour-printing laboratories. No other country has, as yet, put forward any proposals.

With regard to his second question, it is desirable, within the cinema industry, to express the results in a manner which obviates the necessity for any knowledge of photometry, and particularly to eliminate the possibility of confusion between the common procedure of measuring the incident light and the newer one of measuring the brightness. It is for these reasons that we developed the idea of specific brightness and specific colour.

I think it is most important that the standards, when laid down, should be linked to the standard C.I.E. system in order that they may be reproducible, and to allow instruments such as the Morganite Reflectometer to be properly calibrated. I hope, therefore, that the standards, when adopted, will be laid down in terms of the C.I.E. system, but that expression as specific brightness will

continue to be used in kinemas. I hope shortly to express our present datum in terms of the C.I.E. system.

In reply to Mr. Applebee, I agree that maintained lighting, light scattered from the projection beam and the colour of the decorations are not without effect, though usually small enough to be negligible. The maintained lighting installation should be so designed that none falls on the screen, but if the measurements are made with the maintained lighting switched on, any direct effect which they have will be taken into account.

These three causes may have more effect on the picture by contrast than by direct interference, but this is a complex subject, outside the scope of the present paper.

THE MEASUREMENT OF IMPULSE VOLTAGES BY MEANS OF SMALL SPHERE GAPS

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Communicated by Dr. A. P. M. Fleming, 14 *November* 1941

ABSTRACT. The erratic behaviour of small sphere gaps under impulse voltages may be explained by the comparative scarcity of ions in the gap. Methods are described by which this deficiency of ions may be remedied and greater consistency of breakdown be obtained. Suggestions are made for the use of the 0·5-inch diameter sphere gap as a standard for impulse-voltage measurement when irradiated by light from a spark discharge.

§ 1. INTRODUCTION

This investigation arose from an attempt to measure small impulse voltages of very short duration by means of a 0·5-inch-diameter sphere gap. During the measurements it was observed that for the same applied voltage, wide variation was obtained in critical sparking distance and in brightness of spark. Means had therefore to be sought for improving the consistency of the gap, to enable useful calibrations to be made. This could be accomplished by supplying the gap with ionization from an external source, as is described in §§ 3 and 4 below.

§ 2. PROPERTIES OF THE GAP

When impulse voltages are applied to a fixed gap, it is found that between the maximum voltage at which sparkover never occurs and the minimum voltage at which sparkover is always obtained there is an appreciable range in which sparkover occurs intermittently. The impulse breakdown voltage of a gap has been arbitrarily defined in B.S.S. 923/1940 as that voltage for which 50% of the applications will cause breakdown.*

* Afterwards abbreviated to B.D.

The range over which variation is obtained is expressible in terms of "dispersion", which may be defined arbitrarily as:

$$\frac{\text{Voltage required to give 90\% B.D.} - \text{Voltage required to give 10\% B.D.}}{\text{Voltage required to give 50\% B.D.}}$$

(The choice of 90% and 10% is purely one of convenience which has proved to be of good service.)

With small spheres at low spacings, the impulse breakdown voltage may be appreciably higher than the corresponding figure for a constant unidirectional voltage. The *impulse ratio* for any gap is defined as:

$$\frac{\text{Impulse voltage to give 50\% B.D.}}{\text{Constant unidirectional breakdown voltage for same gap}}.$$

Impulse ratio and dispersion are closely related to the time lag of the gap—i.e. to the interval elapsing between the attainment by the impulse of the static breakdown voltage of the gap and the actual breakdown of the gap—which is known to vary with wave-shape. The gap calibration might, therefore, be expected also to depend upon wave-shape.

§ 3. EXPERIMENTAL CALIBRATION OF THE SPARK GAP

The gap under test consisted of two 0·5-inch diameter spheres whose sparking distance could be adjusted to ±0·3 mil by means of a micrometer scale. The test voltage was obtained from a single-stage impulse generator of which the circuit is shown in figure 1. The charging voltage V could be

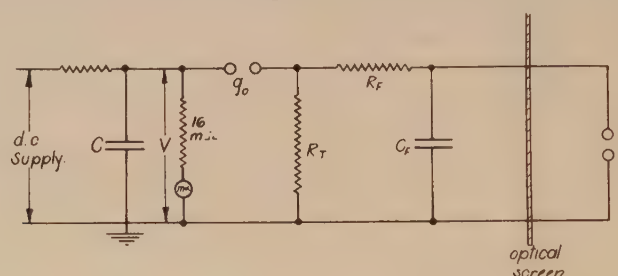

Figure 1. Circuit employed for impulse calibration of sphere gaps.

Circuit constants

C_F μF	R_F ohms	C μF	R_T ohms	Wave-shape μ sec.	Efficiency of generator %
0·0005	100	0·25	25·6	0·15/4·5	96
0·0005	100	0·25	580	0·15/100	100
0·0005	420	0·25	25·6	0·63/4·5	89
0·0005	420	0·25	580	0·63/100	99

measured to within ±0·03 kv. by a resistance voltmeter which consisted of a wire-wound oil-immersed resistance of 18 mΩ in series with a sub-standard milliammeter. The resultant accuracy of measurement was thus in the neighbourhood of ± 6% for a gap of 10 mils, and ± 0·6% for a gap of 100 mils. The impulse voltage was deduced from the charging voltage by computing the

efficiency of the impulse generator from the circuit constants, by the method given in Appendix 2. As no account has been taken of the stray inductance and capacitance of the circuit, the absolute accuracy of the calibration will be somewhat less than the value given above, although comparison of curves taken with the same circuit constants will be unaffected.

Readings were normally taken with negative impulse voltage. A number of readings were repeated with positive impulse voltage at sparking distances up to 100 mils, but any difference due to the change of polarity was less than the probable error of measurement. This is supported by work on larger sphere-gaps (Meador, 1934; Bellaschi and McAuley, 1934), which has shown that polarity difference is first detectable at a spacing of 20% to 25% of sphere diameter.

It was necessary to ensure that the gap g_1 received no radiation from unwanted sources of light, particularly from the gap g_0 of the generator, except where radiation from this source was specified in the calibration. Reflection of light from varnished or polished surfaces had also to be avoided by suitable screening. The spheres were cleaned frequently with metal polish and ether; cleaning appeared to have little effect upon the behaviour of the gap, except in the case of magnesium spheres, when the consistency was impaired by the oxide coating which formed rapidly under sparking. Dust on the cathode might be expected to affect results dependent upon photo-ionization at the metal surface, but seems to be relatively unimportant, probably because in a dusty atmosphere a very large proportion of the precipitation occurs upon the positively charged surfaces.

To enable impulse ratios to be calculated, a calibration of a brass sphere-gap was made up to 10 kv. with constant unidirectional voltage. Radiation from 0·5 mg. of radium was used to give consistency as described in §4 (*a*) I below. No standard calibration tables include the 0·5-inch diameter sphere gap, but a calibration given by Klemm (1923) under radiation from a mercury arc will be seen from table 1 to be in close agreement with that obtained by the author.

Table 1

Separation of spheres (mils)	Static B.D.V. with 0·5 mg. Ra. (kv.)	B.D.V. obtained from curve drawn from Klemm's data (kv.)
10	$1{\cdot}8_1$	$1{\cdot}8_3$
20	$2{\cdot}9_0$	$2{\cdot}9_0$
30	$3{\cdot}8_3$	$3{\cdot}8_4$
40	$4{\cdot}7_3$	$4{\cdot}7_2$
50	$5{\cdot}6_2$	$5{\cdot}6_1$
60	$6{\cdot}5_8$	$6{\cdot}4_8$
80	$8{\cdot}1_7$	$8{\cdot}1_8$
100	$9{\cdot}6_7$	$9{\cdot}8_8$

It should be noted that in all calibrations given here, the voltages have been reduced to their equivalent values at 20° c. and 760 mm. of mercury.

$$V_{20,760} = V_{t \cdot p}/\delta \quad \text{where } \delta = 0{\cdot}386p/(273+t).$$

Figure 2 contains calibration curves taken under various conditions of ionization with an impulse of 0·15/4·5 microseconds, and in figure 3 are shown

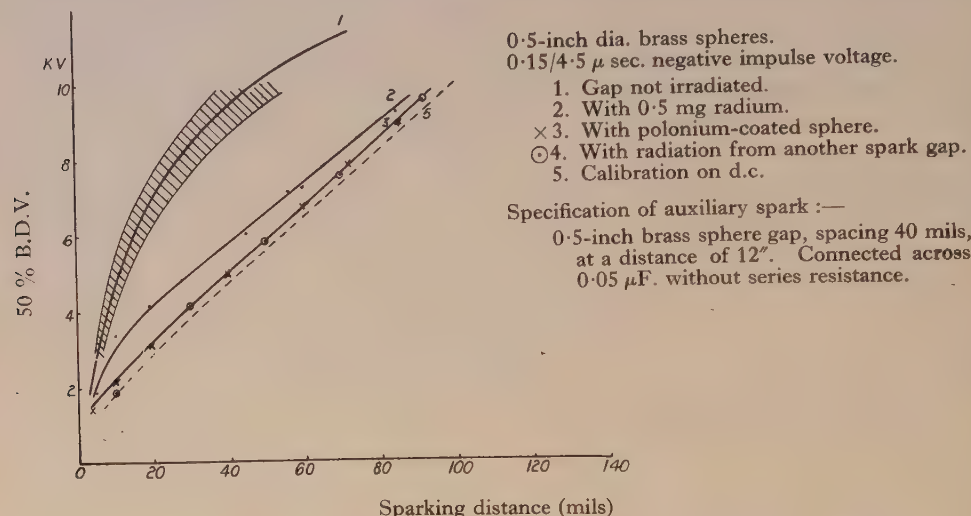

Figure 2. Calibration of 0·5-inch dia. sphere gap with d.c. and impulse voltages. Brass spheres. 0·15/4·5 μ sec. negative impulse voltage.

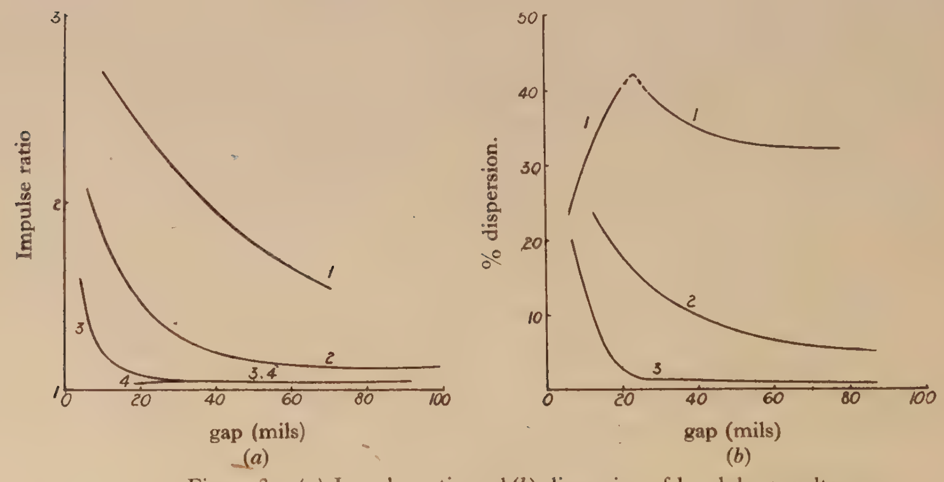

Figure 3. (a) Impulse ratio and (b) dispersion of breakdown voltages.

1. Gap not irradiated.
2. With 0·5 mg. radium.
3. With polonium-coated sphere.
4. With radiation from another spark gap.

the corresponding values of impulse-ratio and dispersion. These results are discussed fully below. Curve 1 was plotted for brass spheres screened from radiation. The hatched area indicates the envelope of points obtained on different occasions. Since results are not repeatable within wide limits, such a calibration is clearly useless for purposes of accurate measurement. If, however, the gap receives a large supply of ions at the time of application of the impulse, much greater consistency is obtained. Ionization may be produced in the gap by the action of

(*a*) the products of disintegration of radioactive substances.

(*b*) ultra-violet radiation from an arc or spark discharge.

§ 4. CALIBRATION OF A GAP SUPPLIED WITH IONIZATION

(*a*) *The use of radioactive substances*

(I) Radium is an ionizing agent frequently employed. In the method devised by Van Cauwenberghe (1930) the radium salt is contained in a small capsule which is inserted in one sphere. Curve 2, figure 2, was obtained with 0·51 mg. of radium element in a container of monel-metal, 0·2 cm. thick, inserted in a brass sphere which had been drilled out to give a wall thickness of 1 mm. of metal between the capsule and the gap. Absorption in the metal would eliminate all but γ radiation, which may be considered as solely responsible for the improvement in the gap. The capsule was found to be equally effective in either anode or cathode. Though the curve shows considerable improvement in the performance of the gap, individual points deviated by 4 % from the mean curve. At spacings of 50 mils or less, dispersion was of the order of 10 % and determination of the 50 % sparkover value required an impracticably lengthy series of readings.

Figure 4 (*a*) shows results obtained with radium as ionizing agent for four different wave-shapes. It will be seen that dispersion and impulse-ratio increase as the impulse tail becomes shorter; this might be expected, as with a longer tail, greater time-lags may be attained and over-voltages will therefore tend to be reduced. Dispersion and impulse ratio are also increased at lower spacings when the wave-front becomes longer. This is possibly because the more slowly rising voltage tends to sweep ions out of the gap before the crest is reached.

(II) Ionization may be provided by active material on the surface of the electrodes (Dillon, 1940). Curve 3, figure 2, was obtained with silver-plated brass spheres, one of which had been coated with a thin film of polonium (for note on intensity of α-particle and γ-ray ionization see Appendix 2). In this case, the emission consisted of α particles with a range of 3 cm. Silver-plated spheres were tested without irradiation and were found to behave in the same erratic manner as brass, therefore it was assumed that the presence of the silver had no effect upon breakdown. It will be seen from the curve that for gaps of 30 mils or more (i.e. at voltages higher than 4 kv.) the dispersion is less than

1 % and the impulse ratio is nearly unity. Results obtained with the polonium coating for other wave-shapes are shown in figure 4 (*b*). The effect of wave-shape upon breakdown voltage, though still evident at lower spacings, is much smaller than when a less powerful ionizing agent is used. It would seem, therefore, that this method of irradiation is suitable for the measurement of impulse voltages above about 4 kv. It should, however, be noted that the

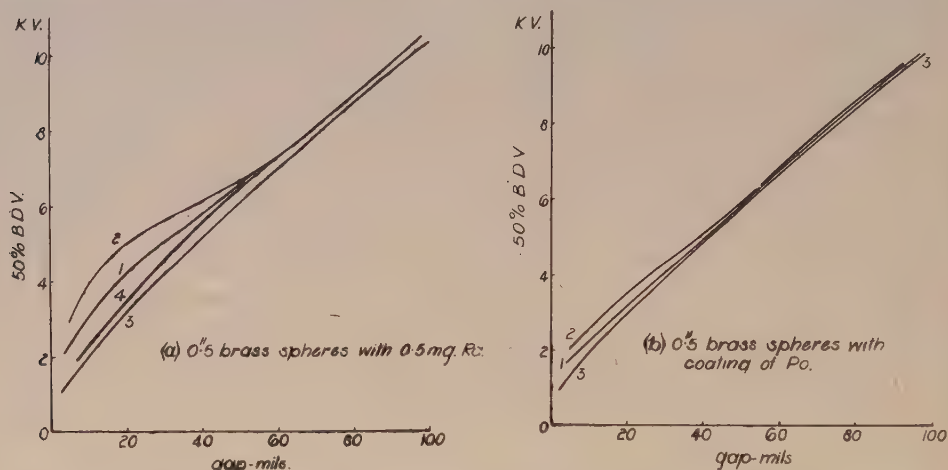

Figure 4. Comparison of B.D. of 0·5-inch sphere gap under impulse voltages of different shapes.
1. 0·15/4·5 μs impulse voltage.
2. 0·63/4·5 ,, ,, ,,
3. 0·15/100 ,, ,, ,,
4. 0·63/100 ,, ,, ,,

polonium coating has a half-life period of 136 days, and will require fairly frequent renewal, though no deterioration of its influence on the calibration was observed 53 days after its preparation. It is also possible that heating of the spheres due to passage of sparks will cause the polonium to volatilize and thus further reduce its useful life.

(*b*) *The use of ultra-violet radiation*

(I) Radiation from a mercury arc in quartz is an ionizing agent that has been adopted by Van Cauwenberghe (1930) and others (Nord, 1935). Results were obtained by the author with a 3-inch arc at a distance of 2 ft. 6 in. from the gap, but as this did not appear to be any more effective than 0·5 mg. of radium and was less convenient mechanically, the method was not pursued further.

(II) The spark discharge itself is known to be a source of considerable ultra-violet radiation, and the effect of irradiation of one spark gap by light from another has been studied by several workers (e.g. Brinkmann, 1939), but the method has not yet been applied to the production of a standard of impulse-voltage measurement.

Two alternatives appear:—

(i) Light from the gap g_0 of the impulse generator may be allowed to fall upon the impulse spark gap, or

(ii) The impulse gap may be illuminated by an independent third gap, and provision made for this gap to operate at the correct instant with respect to the arrival of the impulse.

These two methods are illustrated in figure 7.

The radiation from a spark varies very much in effectiveness, depending upon the voltage across the gap, the values of capacitance and resistance in the discharge circuit, and probably upon the metal of the electrodes.

To obtain repeatable results, therefore, it is essential to specify the circuit of the illuminating spark in detail.

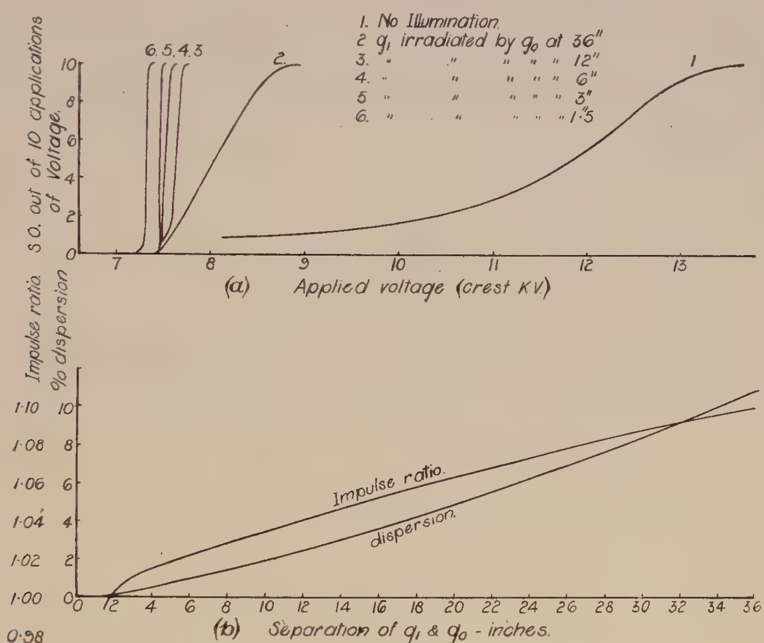

Figure 5. B.D. of 0·5-inch dia. sphere gap under illumination from another spark. Variation of intensity of illumination by adjustment of distance between sparks.

0·15 μsec. negative impulse voltage $g_1=70$ mils.

(i) The curves of figures 5 and 6 were obtained when the impulse-generator gap was used also as the source of illumination. Since the discharge circuit of this gap includes R_T, comparison of results obtained with different waves is not permissible. Figure 5 illustrates the effect of variation of the distance between the two gaps. As the intensity of illumination increases, the width of the dispersion band is reduced until it is negligible, and the impulse ratio approaches unity. Further increase in illumination shifts the curve as a whole

towards the origin, thus causing the 50 % breakdown value to decrease more rapidly, until it attains a value lower than that of the normal d.c. breakdown. In this case the impulse ratio, is apparently less than unity, but this is only a matter of definition of impulse ratio, as illumination of the same intensity would probably reduce the static breakdown voltage to a value equal to or less than the impulse breakdown voltage.

The effectiveness of the spark illumination was reduced appreciably when a barrier of frosted quartz, 0·04 inch thick, was placed between the gaps. When the gaps were 4 inches apart, the quartz increased the breakdown voltage by 1 %, and the dispersion rose from 0·4 % to 0·8 %. With the gaps at a distance of 2 inches, the corresponding increases were 4 % in breakdown voltage and 30 % in dispersion (from 0·3 % to 0·45 %).

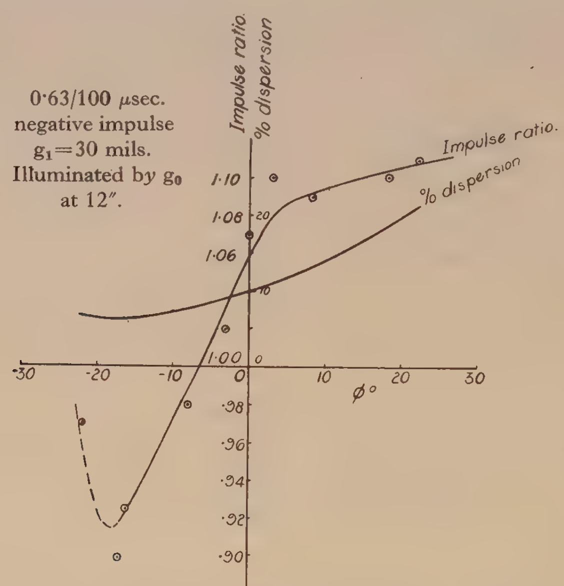

Figure 6. B.D. of 0·5-inch sphere gap under illumination from another spark. Variation of illumination on the cathode.

Figure 6 shows the effect of increasing the proportion of illumination falling on one electrode, while keeping the total illumination constant. The gaps were set up as shown in figure 7 (*a*). The impulse gap was at a fixed distance from the illuminating gap (measured between gap centres), but could be rotated through an angle ϕ, which was taken as positive when the cathode was displaced towards g_0, i.e. when the anode received more illumination. It will be seen from figure 6 that the light is most effective when the angle ϕ is of the order of $-18°$; this is approximately the angle Φ at which the path of the light is along the common tangent to the two spheres, when the cathode receives maximum illumination. These results support the view that the effectiveness of the light

is due to photo-emission from the cathode rather than to volume ionization in the gap.

(ii) When a third gap is used as illuminating source, a very intense spark may be obtained by supplying the gap from a large condenser, without series resistance. Direct comparison of results with different wave-shapes is permissible with this method.

A calibration obtained with illumination from a third gap is shown in curve 4, figure 2. The illuminating gap consisted of brass spheres, spaced at 40 mils (corresponding to 4·7 kv.) and supplied from a condenser of 0·05 μF., at a distance of 12 inches from g_1. To ensure that the impulse gap was illuminated at the instant of arrival of the impulse, it was necessary to allow light from the third gap to trip the impulse generator. This was secured by the arrangement shown in figure 7 (*b*). The voltage V across g_0 was held at a value below the normal

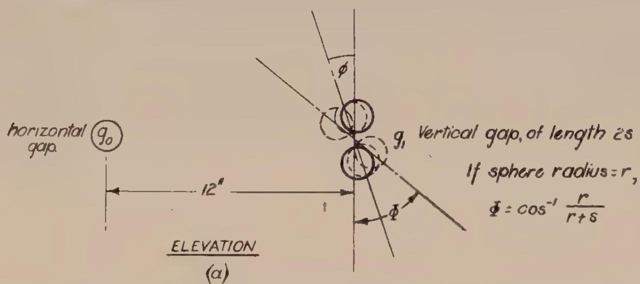

Figure 7 *a*. Illumination of the impulse gap by light from the impulse generator.

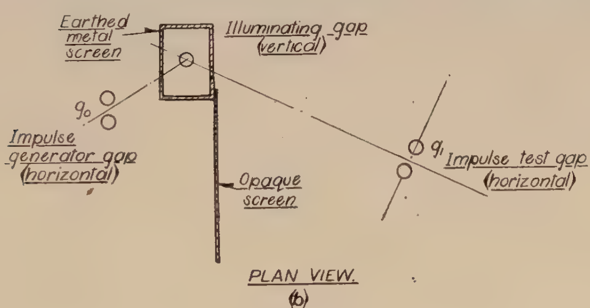

Figure 7 *b*. Illumination of the impulse gap and tripping of the impulse generator by light from a third gap.

breakdown voltage of the gap, but above the breakdown value obtained with intense spark illumination. Passage of a spark at the illuminating gap then caused the generator to trip. To obtain sufficient intensity of illumination at g_0 it was necessary to set it within 6 inches of the illuminating gap. Pick-up in the generator circuit was avoided by surrounding the illuminating gap by an earthed screen, complete except for two small apertures to permit illumination of g_0 and g_1. g_1 was carefully screened from direct light from g_0, though this was probably unimportant, as the spark at g_0 was much less intense than the illuminating

spark. Both electrodes of the impulse gap were equally illuminated, but the influence of small variations in the angle and the distance between the gaps seemed less critical than when the impulse generator gap was used as illuminating agent. It is likely that the relative times of occurrence of maximum intensity of illumination and impulse crest voltage differ in the two methods of spark illumination described here, and that the interval between the two maxima is of importance in determining the influence of the spark.

From figures 2 and 3 it will be seen that the calibration obtained by this method at higher spacings is almost the same as that obtained with polonium, and at lower spacings is of much higher accuracy. Dispersion was negligible over the whole range tested, and the impulse ratio was less than 1·02.

§ 5. SPHERES OF MAGNESIUM ALLOY

The use of electrodes of a metal of low work-function, from which photo-electrons may be liberated under the incidence of normal daylight, was considered as a means of obtaining consistent breakdown. Magnesium has a work-function of 2·7 ev., whereas the values for copper and zinc, of which brass is composed, are 4·0 ev. and 3·4 ev. respectively (these are the values given in Kaye and Laby's *Tables of Physical and Chemical Constants*).

Spheres of magnesium were tested in daylight, but without other radiation. At a gap of 50 mils, dispersions of 25 % and 35 % were obtained with wave tails of 100 and 4·5 microseconds respectively and the impulse ratio was of the order of 1·6 in both cases. Accurate measurement was rendered more difficult by the rapid formation of an oxide film, which, unless cleaning was frequent, rendered the gap almost as inconsistent as one of brass. With brass spheres, the corresponding figures were for long and short tails 33 % and 35 % dispersion, and 1·7 and 1·8 impulse ratio. It seems, therefore, that there is little advantage to be gained by the use of magnesium electrodes.

§ 6. CONCLUSIONS

(1) Small sphere gaps without irradiation are unsuitable for the accurate measurement of impulse voltages of short duration. No advantage is gained by the use of spheres of magnesium.

(2) With 0·5 mg. of radium in the gap, consistency is considerably improved, but is still insufficient to permit of measurements accurate to more than ± 5 % at 5 kv. An improvement of the same order is obtained under radiation from a mercury arc in quartz.

(3) A satisfactory calibration, repeatable to within 1 %, is obtained at voltages of 4 kv. upwards when ionization is obtained from polonium deposited on one of the electrodes. The comparatively short life of the active film is, however, a disadvantage.

(4) If radiation from a spark discharge is employed as an ionizing agent, the consistency of breakdown may be so high that the resultant accuracy of

measurement is governed entirely by the accuracy in setting the measuring gap, provided that the conditions under which the illuminating spark is obtained are reproduced as closely as possible whenever the calibration is used.

§ 7. ACKNOWLEDGEMENTS

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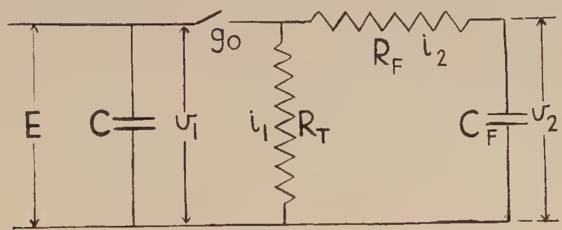
APPENDIX 1

The estimation of correction factors for the determination of impulse voltages from a knowledge of the impulse-generator charging voltage

The impulse generator may be represented schematically as follows:—



If g_0 is closed at time $t=0$, then v_2 is subsequently

$$v_2=\frac{1}{C_F}\int i_2 dt \qquad \ldots\ldots(1)$$

Also

$$R_F i_2+v_2=v_1, \qquad \ldots\ldots(2)$$

$$i_1=v_1/R_T \qquad \ldots\ldots(3)$$

and

$$E=\frac{1}{C}\int (i_1+i_2)dt+v_1. \qquad \ldots\ldots(4)$$

Therefore

$$E=v_1+\frac{1}{C}\left\{\frac{\int v_1 dt}{R_T}+v_2 C_F\right\} \qquad \ldots\ldots(5)$$

and

$$v_1=v_2+R_F\, C_F dv_2/dt. \qquad \ldots\ldots(6)$$

Now, from (1) $$0=Dv_2+R_FC_FD^2v_2+\frac{1}{C}\left(\frac{v_2}{R_T}+\frac{R_FC_F}{R_T}Dv_1+C_FD_Fv_2\right),$$

i.e. $$\left[R_FC_FD^2+\left(1+\frac{C_F}{C}+\frac{R_FC_F}{R_TC}\right)D+\frac{1}{R_TC}\right]v_2=0.$$

Substituting $$T_1=CR_T,\ T_2=C_FR_F,\ X=C_F/C,$$

we have $$\left[T_2D^2+\left(1+X+\frac{T_2}{T_1}\right)D+\frac{1}{T_1}\right]v_2=0.$$

This equation has roots

$$\alpha=\frac{-\left(1+X+\dfrac{T_2}{T_1}\right)}{2T_2}\left[1\pm\sqrt{\left(1-\frac{4T_2}{T_1\left(1+X+\dfrac{T_2}{T_1}\right)^2}\right)}\right].$$

The solution is of the form $v_2=Ae^{-\alpha_1 t}+Be^{-\alpha_2 t}$.

But at $t=0$, $v_2=0$; therefore

$$v_2=A(e^{-\alpha_1 t}-e^{-\alpha_2 t}).$$

Time to crest t_1:— $$Dv_2=A(-\alpha_1e^{-\alpha_1 t}+\alpha_2e^{-\alpha_2 t})$$

$$=0 \text{ when } (\alpha_2-\alpha_1)t=\log_e\frac{\alpha_2}{\alpha_1},$$

i.e., when $$t_1=\frac{\log_e\alpha_2/\alpha_1}{\alpha_2-\alpha_1}.$$

At $t=0$, C_F behaves as a short-circuit,

and $$Dv_2=i_2/C_F=E/T_2,$$

i.e. $$A(-\alpha_1+\alpha_2)=E/T_2,$$

and for unit voltage on C, $$A=\frac{1}{T_2(\alpha_2-\alpha_1)}.$$

Therefore the impulse voltage $v_2=\eta E$, where the correction factor η is given by

$$\eta=A(e^{-\alpha_1t_1}-e^{-\alpha_2t_1})=\frac{e^{-\alpha_1t_1}-e^{-\alpha_2t_1}}{T_2(\alpha_2-\alpha_1)}.$$

APPENDIX 2

The silver sphere was coated with a layer of polonium by immersion in a hot solution of RaD, E and F in ½N HCl for about an hour. Almost half the sphere surface was activated, and the available α-ray intensity was estimated to be $0{\cdot}3\times10^7$ particles/cm²/sec. leaving the sphere. This results in an intensity of ionization of 8×10^{10} ion-pairs/cm³/sec. near the sphere surface. The α-ray intensity from the radium capsule is estimated to be about 4×10^7 ion-pairs/cm³/sec. near the sphere surface, so the intensities differ by a factor 10^3 to 10^4.

THE NATURE OF TEMPERATURE

By W. E. BENHAM

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ABSTRACT. There is at first sight some difficulty in reconciling temperature as energy per unit mass with the conception of temperature obtainable from radiation theory. Evidence is adduced to support the view that the ultimate significance of temperature is that it is measured by the thinness of a pulse of electromagnetic radiation. Some preliminary remarks on a new theory of radiation (which requires that central orbits shall be non-radiating when circular) are used to support a dimensional treatment in which the energy density of radiation is a function of the mass (rather than of the charge) of an electron and of the absolute temperature. The actual dimensions of temperature appear to be 1/L, and this is reconciled with energy per unit mass by attributing dimensions to " Newton's constant " N. It turns out that this and the gravitational constant have the dimensions

$$[N]=\mathrm{T}^2/\mathrm{L}^3,$$
$$[G]=1/\mathrm{M}.$$

The Wiedemann-Franz ratio has dimensions M/L², and a number of thermal quantities are listed as to their dimensions on the basis proposed. A quantity known to be an adiabatic invariant has the dimensions T/L and the velocity involved is thought to be that of electromagnetic waves.

§ 1. INTRODUCTION

LORD KELVIN'S proposal to eliminate mass as a fundamental unit was criticized in a recent discussion,* the keynote of which is the retention of what will be termed " Newton's constant " in the mass-acceleration law

$$F=Nma. \qquad \ldots\ldots(1)$$

While N may be taken for convenience as of unit magnitude, this makes it too easy to forget the dimensions of N, which should therefore be retained until valid reasons arise for doing otherwise.

We shall now take temperature as energy per unit mass, understandable from the kinetic theory and from calorimetry with heat as a form of energy. If we do not take N as dimensionless, we no longer find that temperature has the dimensions [velocity]², or, writing θ for absolute temperature, T being wanted for the dimensions of time, Θ being the dimensions of temperature, M, L those of mass, length, respectively, we do not find

$$\Theta=\mathrm{L}^2/\mathrm{T}^2 \qquad \ldots\ldots(2)$$

but

$$\Theta=[N]\mathrm{L}^2/\mathrm{T}^2. \qquad \ldots\ldots(3)$$

* *Proc. Phys. Soc.* 1940, **52**, 608.

We shall show that the retention of N, that is, the retention of generality so dear to mathematical physicists, except, apparently, when the subject is that of "dimensions", is of assistance in clearing up an apparent discrepancy in the dimensions of temperature as derived above, on the one hand, and as obtained either from the classical theory of radiation by collisions or from the principle of adiabatic invariance, used in the quantum theory of radiation, on the other hand. Let us first see where Kelvin's logic will lead us. He says, in effect, that if we measure in units for which G, the gravitational constant, as well as N, is dimensionless, the proper measure of a force is the fourth power of a linear velocity, while mass per unit volume is measured by the square of an angular velocity, or

$$\mathrm{F}=\mathrm{L}^4/\mathrm{T}^4\ ;\quad \mathrm{M}=\mathrm{L}^3/\mathrm{T}^2. \qquad \ldots\ldots(4)$$

Now, whilst it is one thing to take some quantity or other as the proper measure of a physical entity, it is quite another to identify the dimensions of the entity with those of the quantity, as is done under (4). Thus, $\mathrm{L}^4/\mathrm{T}^4$ has been taken as the physical dimensions of force. Once we start doing this sort of thing, there is no knowing where we shall end up. We might just as well say that because the energy density of black-body radiation varies as the fourth power of the absolute temperature and is independent of the material giving rise to the radiation, therefore Θ^4 will represent the physical dimensions of either energy per cubic centimetre or energy per square cm. per sec., which leads to two possibilities:—

$$\Theta^4=\mathrm{M}/\mathrm{T}^2\mathrm{L}\ ;\quad \Theta^4=\mathrm{M}/\mathrm{T}^3, \qquad \ldots\ldots(5)$$

neither of which is reconcilable with (2), to which Kelvin is logically committed, or, saving fractional indices for Θ, with the second of (4).

§ 2. DISCUSSION OF STEFAN'S LAW

Let us begin by calling attention to the formula of classical electromagnetic theory for the energy per unit volume of a stream of radiant energy at a place where the dielectric constant * is k and the electric vector has the value E, namely,

$$\epsilon=kE^2/4\pi. \qquad \ldots\ldots(6)$$

Equation (6) is valid in any set of units provided k is appropriately expressed, but we shall regard it as being in electrostatic units. The energy due to the magnetic vector of the field is of course included in (6), being equal in magnitude to half the total.

Now the electric vector arises, in the last analysis, from electric charges. In the study of (6) from the dimensional standpoint we need only concern ourselves with this qualitative statement. Replacing E in terms of charges of dimensions Q, but of unknown or unspecified magnitude or location, we have the dimensional equation (where K denotes the dimensions of k)

$$[\epsilon]=\mathrm{Q}^2/\mathrm{KL}^4. \qquad \ldots\ldots(7)$$

* Regarded as including any possible "force constant".

We next point out that the above dimensional relation must hold good in the particular case of black-body radiation, for which ϵ varies as θ^4. Now, in the classical theory of radiation by collisions, it turns out that the time occupied by a collision of an electron with a molecule is inversely proportional to the absolute temperature. To quote Sir J. J. Thomson (1907a): " The mathematical theory of the production of radiation by collisions shows that this energy (the usual form of which is $\lambda^{-5}\phi(\lambda\theta)d\lambda$) is given by an expression of the form

$$\lambda^{-5}F(\lambda/VT)d\lambda, \qquad \ldots\ldots(8)$$

where T is the duration of a collision, V the velocity of light and F represents a function whose form depends upon the nature of the forces exerted during a collision. Comparing these two expressions we see that T must be inversely proportional to θ, that is, inversely proportional to the square of the velocity of the corpuscles. The velocity of corpuscles at 0° C. when in temperature equilibrium with their surroundings is about 10^7 cm./sec.; the wave-length at which the intensity is greatest at 0° C. is about 10^{-3} cm. In a Röntgen-ray bulb giving out hard rays, the velocity of the corpuscles may be about 10^{10} cm./sec.—or 10^3 times those in the metal; hence, if the law of duration of impacts is true, the radiation produced by the impact of the corpuscles in the tube should be a maximum for a wave-length of $10^{-3}/10^6$ or 10^{-9} cm., and [Thomson had "as"] this is of the same order as the thickness of a pulse of very penetrating Röntgen radiation; this test, as far as it goes, confirms the law of "duration of collisions". The thickness of the pulse resulting from the collision, being the distance travelled by electromagnetic radiation during the time of a collision, will also be proportional to $1/\theta$, and it is perhaps significant that if we make use of this proportionality in (7), that is, if we replace 1/L by θ, we obtain the result that ϵ is proportional to θ^4.

Thomson's conclusions were supported by the work of Jeans (1914 and 1924 a), who passed them under critical review, the conclusion being that they lead to conditions inside radiating matter most difficult to reconcile with known facts. However, this does not affect the argument which matters here, namely, that when we approach the question of the dimensions of temperature from the laws of radiation as starting point, we find that the important variable in determining changes of θ is L or T, occurring singly, rather than L/T. This result begins to suggest itself at equation (7). Thus, for radiation from a substance the specific inductive capacity of which does not vary with temperature, the only variables in (7) are L and Q. If, for example, the charge of which Q denotes the dimensions is that of a single electron, the fact that temperature influences the effective number of charges being taken care of by supposing that L includes the variable distance between electrons, then L is the sole variable. The details are unimportant for a dimensional discussion, but it may be of interest to point out that on reasonable assumptions Sir J. J. Thomson (1907 b) has included the effects of radiation between collisions, as well as at the collisions themselves, and while the latter would no doubt have been uppermost in his

mind as the chief cause of radiation, he did not omit to take into account, in his second analysis of the connection between radiant energy and temperature, the part played by the path between collisions. Thus, for a dimensional discussion, L might equally well be thought of as the mean free-path, which, as Thomson points out, does in fact vary approximately as the reciprocal of the absolute temperature in the several pure metals for which the temperature coefficient of resistance is in the neighbourhood of 1/273 (Thomson 1907 c).

Let us now proceed to a more rigorous dimensional discussion. Let us suppose the temperature enters as θ^4 in known manner, but let us include the possibility that charge enters as Q^α. Let A be a purely numerical constant, and let us assume for the moment that $k=1$. Then

$$\epsilon = A\,q^\alpha \theta^4 \qquad \text{.......(9)}$$

or, dimensionally,

$$M/LT^2 = Q^\alpha \Theta^4, \qquad \text{......(9 a)}$$

giving

$$\Theta^4 = M^{1-\frac{\alpha}{2}} L^{-1-\frac{3\alpha}{2}} T^{\alpha-2}. \qquad \text{......(10)}$$

The only possible value of α which makes Θ independent of M is 2, for which

$$\Theta = L^{-1}. \qquad \text{......(11)}$$

Now this will *not* do, though it gives the correct result, as we shall see. One reason is that we have omitted to include on the left-hand side of (9 ***a***) the quantity [N]. Moreover, we have made no provision for the possibility that the mass of the exciting charge is an important factor. The justification (Thomson 1907 **d**) for the omission of M, namely, " that the expressions . . . . for the electrical and thermal conductivities, the radiation, and the other electrical effects do not involve the mass of the carrier, so that the results would hold if the carriers were bodies having a much greater mass than that of a corpuscle ", is a very strong one if we accept the classical theory of radiation. If, however, we accept the quantum theory, neither mass nor charge, but $\boldsymbol{c}$, $\boldsymbol{h}$, and $\boldsymbol{k}$, enter into the expression (O'Rahilly, 1938).

The writer has in hand a theory which seems to give radiation from uncharged as well as charged bodies. The theory has had to be shelved during the war, but for present purposes the following result is all that we require. The rate at which any mass m, whether charged or no, radiates energy is given by

$$\partial U/\partial t, \qquad \text{......(12)}$$

where $-U$ is the potential energy of the mass in the field of force, and $\partial/\partial t$ denotes differentiation at a fixed point of space,—a local rate of change with the time. In case the mass m is without charge, U contains m, while if the field of force is electric, U contains m through the square of the electric charge. Thus we adhere to the Thomson-Lorentz view of electromagnetic mass, but regard mass rather than charge as the entity which is necessary for radiation. This means that in those applications for which Larmor's formula for radiation from an accelerated point-charge is reconcilable with observation, the rate of radiation

may be expressed in terms of the radius a of the " point " charge, and of its mass:

$$m = 2q^2/3ac^2, \qquad \text{......(13)}$$

giving

$$\partial R/\partial t = 2q^2f^2/3c^3 = maf^2/c, \qquad \text{......(14)}$$

where R is the flux of radiation and f the acceleration of the particle.

We thus make a fresh start; instead of (9) we try ($\boldsymbol{m}$ = electronic mass)

$$\epsilon = A\boldsymbol{m}^{\alpha}\theta^4. \qquad \text{......(15)}$$

This time we include N, so that our dimensional equation is

$$[N]\mathrm{M}/\mathrm{LT}^2 = \mathrm{M}^{\alpha}\Theta^4, \qquad \text{......(16)}$$

giving, if $\alpha = 1$,

$$\Theta^4 = [N]/\mathrm{LT}^2. \qquad \text{......(17)}$$

Eliminating $[N]$ between (3) and (17) we obtain the result already reached under false assumptions at (11). The dimensions of N are thus

$$[N] = \mathrm{T}^2/\mathrm{L}^3. \qquad \text{......(18)}$$

We see that by including N, and only by so doing, we have been able to reconcile (11) with the generally agreed view that temperature is energy per unit mass, generally expressed by (2), but correctly expressed by (3).

It may be argued that we had no right to take $\alpha = 1$ in reaching (17), since N is of unknown dimensions. However, N is almost certain not to contain M, and if θ is to be free of M, $\alpha = 1$ is the only alternative. If we admit the possibility that either θ or N may contain M, (16) and (3) give

$$\Theta = \mathrm{M}^{(1-\alpha)/3}/\mathrm{L}\ ; \quad [N] = \mathrm{M}^{(1-\alpha)/3}\mathrm{T}^2/\mathrm{L}^3. \qquad \text{......(19)}$$

This allows those who wish to make G (the gravitational constant) dimensionless to do so, as follows. The dimensions of G are, using (19),

$$[G] = \mathrm{L}^3[N]/\mathrm{MT}^2 = \mathrm{M}^{(-2-\alpha)/3}. \qquad \text{......(20)}$$

The choice is thus

$$\alpha = -2. \qquad \text{......(21)}$$

This is not a choice favoured by the writer, since it involves

$$\epsilon = A\theta^4/\boldsymbol{m}^2\ ; \quad \Theta = \mathrm{M}/\mathrm{L}, \qquad \text{......(22)}$$

which has no very obvious physical significance. Equation (11), however, has the immediate significance that temperature is measured by the thickness of a pulse of electromagnetic radiation (the fact that we do not make the measurement is immaterial). It should be emphasized that since the mass of the carrier is the same for all substances, assuming all the radiation due to electrons, the appearance of $\boldsymbol{m}$ in (15) is without prejudice to the experimental result that the energy density of black-body radiation is independent of the atomic weight of the substance.

It may be noticed that, according to (12),

(*a*) Acceleration is a necessary, but not a sufficient, condition for radiation.

(*b*) In particular, U would be constant for an electron moving in a circular orbit under a central force, so that despite centripetal acceleration no radiation would result. This is in accordance with observation, for the width of spectral lines is finite, which implies that the orbits are elliptical. Thus, according to the writer's theory, orbits are all circular until atoms are in some way excited, when they are distorted into ellipses. This need not necessarily conflict with the quantum theory, since there would appear to be no experiment which could establish the existence of elliptical orbits in atoms while not actually radiating. Actually, if it were not for precession of elliptical orbits (generally attributed to relativistic changes in mass) it would be possible for (12) to apply to elliptical as well as to circular orbits, for (12) states in this connection that every time the electron comes round to the same point of its orbit it finds the same field acting upon it. This condition could not be satisfied by a precessing orbit.

The above remarks are largely incidental to the discussion. We conclude this section by evaluating A in (15). If $\boldsymbol{\sigma}$ is Stefan's constant $(=5{\cdot}74\times10^{-5})$, we have

$$\epsilon=(4\boldsymbol{\sigma}/\boldsymbol{c})\theta^4. \qquad \ldots\ldots(23)$$

Comparing with (15), taking $\alpha=1$:

$$A=4\boldsymbol{\sigma}/\boldsymbol{mc}. \qquad \ldots\ldots(24)$$

With $\boldsymbol{m}=9{\cdot}09/10^{-28}$, $\boldsymbol{c}=3{\cdot}00\times10^{10}$,

$$A=8{\cdot}42\times10^{12}. \qquad \ldots\ldots(25)$$

The value of A in (9) would have been $3{\cdot}32\times10^4$.

If the dimensions of temperature are the reciprocal of a length, then Stefan's and Boltzmann's constants have the dimensions of momentum and mass respectively:

$$[\boldsymbol{\sigma}]=[N]\mathrm{ML}^4/\mathrm{T}^3=\mathrm{ML}/\mathrm{T}\ ; \quad [\boldsymbol{k}]=[N]\mathrm{ML}^3/\mathrm{T}^2=\mathrm{M}. \qquad \ldots\ldots(26)$$

It is hardly necessary to add that when these dimensions are inserted in the quantum formula

$$\boldsymbol{\sigma}=2\pi^5\boldsymbol{k}^4/15\boldsymbol{c}^2\boldsymbol{h}^3 \qquad \ldots\ldots(27)$$

the dimensions of $\boldsymbol{h}$ come out as those of action, namely, $[N]\mathrm{ML}^2\mathrm{T}^{-1}=\mathrm{MT}/\mathrm{L}$.

§ 3. THE WIEDEMANN-FRANZ RATIO AND SPECIFIC HEAT

The Wiedemann-Franz ratio is unaffected by the inclusion of N, i.e. it is still given by

$$k/\sigma=3\boldsymbol{k}^2\theta/\boldsymbol{e}^2\mathcal{J}; \qquad \ldots\ldots(28)$$

expressing $\boldsymbol{e}$ in e.m.u. and remembering (18),

$$[k/\sigma]=\mathrm{M}/\mathrm{L}^2; \qquad \ldots\ldots(29)$$

the appearance of M is not of great significance, but it matters that we have L^2 in

the denominator. This rather suggests that the ratio should vary as the square of the absolute temperature. The work of Meissner (see Roberts, 1928) suggests that at very low temperatures this may be true.

The thermal capacity of a body may be defined as (the number of heat units required to raise its temperature by $d\theta$) divided by $d\theta$. As usually defined, specific heat is a pure number, a ratio, and the question of its dimensions does not arise. What matters in the present connection is that thermal capacity per unit mass, to which the term specific heat is frequently, if loosely, applied, has the dimensions

$$\frac{\mathrm{H}}{\mathrm{M}\Theta}=[N]\mathrm{ML^2T^{-2}}/\mathrm{M}\Theta, \qquad \ldots\ldots(30)$$

where H refers to the quantity of heat. As in connection with (28), we have taken the mechanical equivalent of heat, J, as dimensionless and have expressed H in work units. We thus have, in view of (19), which gives

$$[N]/\Theta=\mathrm{T^2/L^2}, \qquad \ldots\ldots(31)$$

that thermal capacity per unit mass is, like specific heat, without dimensions. This circumstance must be regarded as fortunate in view of the widespread identification of these two quantities.

The dimensions of various quantities on the new basis are listed below:—

Temperature	1/L
Quantity of Heat	M/L
Thermal Capacity (per unit mass)	1
Thermal Capacity (per unit volume) ...	$\mathrm{M/L^3}$
Thermal Conductivity	M/LT
Entropy	M
Enthalpy	M/L

§ 4. ADIABATIC INVARIANCE*

If F be a quantity satisfying the equation

$$F=n\boldsymbol{h}, \qquad \ldots\ldots(32)$$

where n is an integer, and if a be a quantity which changes with any of the external fields of force acting on a dynamical system describing a motion conforming to the quantum condition (32), then in order that (32) may be a quantum equation in the general case when a may have any value whatever,

$$\partial F/\partial a=0. \qquad \ldots\ldots(33)$$

The function $2\overline{T}/\nu$ was shown by Boltzmann (1876) to be an adiabatic invariant (that is, to satisfy (33)).

Here $\overline{T}$ is the average kinetic energy over a period of an oscillation of frequency ν. On our present basis, all this means is that the velocity of light

* See Jeans, 1924, op. cit. p. 65.

is an adiabatic invariant. For, regarding $\overline{T}$ as reckoned per unit mass, the dimensions of $\overline{T}$ are $1/L$, the same as those of temperature, so that the dimensions of $2\overline{T}/\nu$ are those of the reciprocal of a velocity. This velocity could not possibly refer in general to the velocity of any of the mechanical parts of the system; the only possible velocity which is invariant being that of electromagnetic waves *in vacuo*, this must be the velocity concerned.

§ 5. ACKNOWLEDGEMENT

I should like to thank Professor G. Stead for his interest and valued criticism.

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A PROBLEM IN TWO-DIMENSIONAL FLOW

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ABSTRACT. A method of solving a particular case of the two-dimensional heat-flow equation is given and used to calculate the distribution of moisture content in a clay cylinder drying from its curved surface only. The results of such calculations are compared with experimental data.

§ 1. INTRODUCTION

It has been previously shown (Macey, 1940) how it was possible to calculate the distribution of moisture-content in clay bars drying at a constant rate from one end only. The ultimate objective of this work being the correlation of the moisture distribution with the appearance of cracks during the drying process, it was necessary to develop the theory into two or three dimensions. For practical reasons, notably the difficulties of supporting a sphere of wet clay and of ensuring uniform drying over its surface, the cylinder drying only from its curved surface is to be preferred. It was therefore required to know the moisture-content distribution at any time in such a cylinder drying at a

constant rate. The problem involved is essentially similar to that of heat flow, and it may be that the method of solution adopted, though unorthodox, is capable of application in other fields.

§ 2. ONE-DIMENSIONAL FLOW

If a clay be subjected to a pressure P between porous pistons, it assumes a moisture-content (percentage on basis of dry weight of clay) M such that $P=\alpha e^{-\beta M}$, α and β being constants for any particular batch of clay. The permeability or aqueous conductivity, C, is a similar exponential function of moisture-content, $C=\theta e^{\phi M}$, θ and ϕ being again experimentally determined constants. On the assumption that the water movement is to be associated with the pressure P, the basic differential equation for linear flow is

$$\frac{\partial}{\partial x}\left[C\frac{\partial P}{\partial x}\right]=\frac{\partial}{\partial t}\left[\frac{M}{S+M}\right], \qquad \ldots\ldots(1)$$

where S is the volume of 100 grams of clay in the plastic mixture. For a detailed argument, reference must be made to the original paper, but since, experimentally, ϕ and β are approximately equal, this is reduced to

$$\alpha\beta\theta e^{(\phi-\beta)M_c}(S+M)\frac{\partial^2 M}{\partial x^2}=\frac{\partial M}{\partial t}. \qquad \ldots\ldots(2)$$

This is the familiar heat-flow equation which, by suitable adjustment of the units of time, is further reduced to

$$\frac{\partial^2 M}{\partial x^2}=\frac{\partial M}{\partial t}. \qquad \ldots\ldots(3)$$

The boundary conditions are that the moisture-content is initially uniform, that the rate of drying at the surface is constant, and that the moisture-content gradient at the non-drying end of the bar is always zero, i.e. $M=M_0$ when $t=0$, $\partial M/\partial x=A$ at the surface, and $\partial M/\partial x=0$ at the non-drying end. A solution suitable for small values of the time t is

$$M=M_0-2At^{\frac{1}{2}}\left[\chi_1\left(\frac{x}{2t}\right)+\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)+\chi_1\left(\frac{2l+x}{2t^{\frac{1}{2}}}\right)+\chi_1\left(\frac{4l-x}{2t^{\frac{1}{2}}}\right)\right.$$
$$\left.+\chi_1\left(\frac{4l+x}{2t^{\frac{1}{2}}}\right)+\ldots\right], \qquad \ldots\ldots(4)$$

where the origin is taken at the drying surface and χ_1 is the first integral of the error-function complement as defined by Hartree (1936). The error-function as defined by Jeffreys (1927) being

$$\operatorname{erf}\omega=\frac{2}{\sqrt{\pi}}\int_0^\omega e^{-\xi^2}\,d\xi, \qquad \ldots\ldots(5)$$

Hartree takes the complement to be the function

$$\chi(\omega)=\frac{2}{\sqrt{\pi}}\int_\omega^\infty e^{-\xi^2}\,d\xi, \qquad \ldots\ldots(6)$$

and repeated integrals are defined as

$$\chi_n(\omega)=\int_\omega^\infty \chi_{n-1}(\xi)\,d\xi, \qquad \ldots\ldots(7)$$

with $\chi_0(\omega)=\chi(\omega)$ as in (6).

A second solution suitable for large times is

$$M = M_0 - \frac{A}{l}\left[\left(t + \frac{x^2}{2} - \frac{l^2}{6}\right) - \frac{2l^2}{\pi^2}\sum_{n=1}^{\infty}(-1)^n \frac{1}{n^2}\cos\frac{n\pi x}{l} \cdot e^{-n^2\pi^2 t/l^2}\right], \quad \ldots\ldots(8)$$

in which the origin is taken at the non-drying end of the bar. For large values of t, this reduces to

$$M = M_0 - \frac{A}{l}\left(t + \frac{x^2}{2} - \frac{l^2}{6}\right). \quad \ldots\ldots(9)$$

These forms of solution are highly convenient, for when more than two terms of (4) are necessary, the simple solution (9) holds, and it is never required to evaluate the summation term in (8).

§ 3. TWO-DIMENSIONAL FLOW

With the same basic theory, and taking the origin at the centre, the differential equation in the case of a cylinder may be written

$$\alpha\beta\theta\, e^{(\phi-\beta)M_0}(S+M)\frac{\partial}{dx}\left[x\frac{\partial M}{dx}\right] = x\frac{\partial M}{\partial t}, \quad \ldots\ldots(10)$$

which, by adjustment of the units of time as before, reduces to

$$\frac{\partial}{\partial x}\left[x\frac{\partial M}{\partial x}\right] = x\frac{\partial M}{\partial t}. \quad \ldots\ldots(11)$$

A strict method of solving this, under the same boundary conditions, is unknown.

A further condition to be observed is that the total change of moisture-content at any time must be equal to the loss of water by drying, i.e.,

$$\pi M_0 l^2 - 2\pi\int_0^l Mx\,dx = 2\pi A l\tau, \quad \ldots\ldots(12)$$

where τ is the time of drying of the cylinder of radius l.

A particular solution, valid for large times, and satisfying all these conditions, is

$$M = M_0 - \frac{A}{l}\left[2\tau + \frac{x^2}{2} - \frac{l^2}{4}\right]. \quad \ldots\ldots(13)$$

Compare this with the solution (9) for a bar of length l. It is at once apparent that they are identical if

$$2\tau - \frac{l^2}{4} = t - \frac{l^2}{6}.$$

In both the bar of length l and the cylinder of radius l, drying at the same rate, the moisture-content is initially uniform at M_0. At large times, the curves of moisture-content distribution are identical parabolas *on a different time scale*, which do not change in shape, but only in position along the moisture-content scale with time. The solution for small times now rests upon the following argument: if two families of curves of similar derivation, both subject to the same boundary conditions which define the constant values of their slopes at their limits, are initially straight lines and are ultimately identical parabolas, then they must have the same evolution. The solution (4) for linear flow must also hold for the cylinder, but at some time other than t. Knowing the time

of drying τ of a cylinder of radius l, the problem reduces to that of finding the equivalent time of drying t of a bar of length l, when that bar will have the same moisture-content distribution as the cylinder.

This can be done by using the condition (12) and, transferring the origin to the surface of the cylinder, we have

$$2\pi l A\tau = 4\pi\int_0^l At^{\frac{1}{2}}\left[\chi_1\left(\frac{x}{2t^{\frac{1}{2}}}\right)+\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)+\chi_1\left(\frac{2l+x}{2t^{\frac{1}{2}}}\right)+\dots\right](l-x)\,dx. \qquad \dots\dots(14)$$

The integral is derived as follows:

We have, integrating by parts,

$$\int_\omega^\infty \xi\chi_{n-1}(\xi)d\xi = \omega\chi_n(\omega)+\chi_{n+1}(\omega), \qquad \dots\dots(15)$$

and in particular

$$\int_\omega^\infty \xi\chi_1(\xi)d\xi = \omega\chi_2(\omega)+\chi_3(\omega).$$

Hence if ξ is a function of x

$$\int_{x=0}^{x=l} \xi\chi_1(\xi)d\xi = \Big[\xi\chi_2(\xi)+\chi_3(\xi)\Big]_{x=l}^{x=0}. \qquad \dots\dots(16)$$

It is required to evaluate

$$2t^{\frac{1}{2}}\int_0^l\left[\chi_1\left(\frac{x}{2t^{\frac{1}{2}}}\right)+\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)+\chi_1\left(\frac{2l+x}{2t}\right)+\chi_1\left(\frac{4l-x}{2t^{\frac{1}{2}}}\right)+\dots\right](l-x)dx. \qquad \dots\dots(17)$$

Now

$$2t^{\frac{1}{2}}l\int_0^l\left[\chi_1\left(\frac{x}{2t}\right)+\chi_1\left(\frac{2l-x}{2t}\right)+\dots\right]dx$$

$$=4tl\left[\chi_2\left(\frac{x}{2t^{\frac{1}{2}}}\right)-\chi_2\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)+\chi_2\left(\frac{2l+x}{2t^{\frac{1}{2}}}\right)+\dots\right]_l^0$$

$$=4tl\,\chi_2(0)$$

$$=tl \qquad \dots\dots(18)$$

since $\qquad \chi_2(0)=\frac{1}{4}$.

To evaluate the second term of (17) put $\xi = x/2t^{\frac{1}{2}}$ in (16).

Then

$$\int_0^l\left(\frac{x}{2t^{\frac{1}{2}}}\right)\chi_1\left(\frac{x}{2t^{\frac{1}{2}}}\right)\left(\frac{dx}{2t^{\frac{1}{2}}}\right)=\left[\left(\frac{x}{2t^{\frac{1}{2}}}\right)\chi_2\left(\frac{x}{2t^{\frac{1}{2}}}\right)+\chi_3\left(\frac{x}{2t^{\frac{1}{2}}}\right)\right]_l^0$$

or

$$\int_0^l x\chi_1\left(\frac{x}{2t^{\frac{1}{2}}}\right)dx = 4t\left[\chi_3(0)-\left(\frac{l}{2t}\right)\chi_2\left(\frac{l}{2t^{\frac{1}{2}}}\right)-\chi_3\left(\frac{l}{2t^{\frac{1}{2}}}\right)\right]. \qquad \dots\dots(19)$$

Similarly, putting $\xi=\left(\dfrac{2l-x}{2t^{\frac{1}{2}}}\right)$ in (16), we find

$$\int_0^l\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)\left(\frac{dx}{2t^{\frac{1}{2}}}\right)=\left(\frac{l}{2t^{\frac{1}{2}}}\right)\chi_2\left(\frac{l}{2t^{\frac{1}{2}}}\right)$$

$$+\chi_3\left(\frac{l}{2t^{\frac{1}{2}}}\right)-\left(\frac{2l}{2t^{\frac{1}{2}}}\right)\chi_2\left(\frac{2l}{2t^{\frac{1}{2}}}\right)-\chi_3\left(\frac{2l}{2t^{\frac{1}{2}}}\right). \qquad \dots\dots(20)$$

But

$$\int_0^l \left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)\frac{dx}{2t^{\frac{1}{2}}} = \frac{1}{4t}\int_0^l 2l\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)dx - \frac{1}{4t}\int_0^l x\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)dx$$

and

$$\frac{1}{4t}\int_0^l 2l\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)dx = -\left(\frac{2l}{2t^{\frac{1}{2}}}\right)\left[\chi_2\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)\right]_l^0 = \left(\frac{2l}{2t^{\frac{1}{2}}}\right)\chi_2\left(\frac{l}{2t^{\frac{1}{2}}}\right) - \left(\frac{2l}{2t^{\frac{1}{2}}}\right)\chi_2\left(\frac{2l}{2t^{\frac{1}{2}}}\right). \qquad \ldots\ldots(21)$$

Therefore

$$\frac{1}{4t}\int_0^l x\chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right)dx = \left(\frac{l}{2t^{\frac{1}{2}}}\right)\chi_2\left(\frac{l}{2t^{\frac{1}{2}}}\right) + \chi_3\left(\frac{2l}{2t^{\frac{1}{2}}}\right) - \chi_3\left(\frac{l}{2t^{\frac{1}{2}}}\right). \qquad \ldots\ldots(22)$$

Similarly, put $\xi = \left(\frac{2l+x}{2t^{\frac{1}{2}}}\right)$. Then

$$\frac{1}{4t}\int_0^l x\chi_1\left(\frac{2l+x}{2t^{\frac{1}{2}}}\right)dx = \chi_3\left(\frac{2l}{2t^{\frac{1}{2}}}\right) - \left(\frac{l}{2t^{\frac{1}{2}}}\right)\chi_2\left(\frac{3l}{2t^{\frac{1}{2}}}\right) - \chi_3\left(\frac{3l}{2t^{\frac{1}{2}}}\right). \qquad \ldots\ldots(23)$$

In the same manner

$$\frac{1}{4t}\int_0^l x\chi_1\left(\frac{4l-x}{2t^{\frac{1}{2}}}\right)dx = \chi_3\left(\frac{4l}{2t^{\frac{1}{2}}}\right) + \left(\frac{l}{2t^{\frac{1}{2}}}\right)\chi_2\left(\frac{3l}{2t^{\frac{1}{2}}}\right) - \chi_3\left(\frac{3l}{2t^{\frac{1}{2}}}\right), \qquad \ldots\ldots(24)$$

and similarly for subsequent terms.

Adding (19), (22), (23), (24),

$$\int_0^l \left[\chi_1\left(\frac{x}{2t^{\frac{1}{2}}}\right) + \chi_1\left(\frac{2l-x}{2t^{\frac{1}{2}}}\right) + \chi_1\left(\frac{2l+x}{2t^{\frac{1}{2}}}\right) + \ldots\right]x\,dx$$

$$= 4t\left[\chi_3(0) - 2\chi_3\left(\frac{l}{2t^{\frac{1}{2}}}\right) + 2\chi_3\left(\frac{2l}{2t^{\frac{1}{2}}}\right) - 2\chi_3\left(\frac{3l}{2t^{\frac{1}{2}}}\right) + \ldots\right]$$

$$= 4t\left[\chi_3(0) + 2\Sigma(-1)^n\chi_3\left(\frac{nl}{2t^{\frac{1}{2}}}\right)\right]. \qquad \ldots\ldots(25)$$

Hence, from (14), (17), (18) and (25),

$$l\tau = lt - 8t^{3/2}\left[\chi_3(0) + 2\Sigma(-1)^n\chi_3\left(\frac{nl}{2t^{\frac{1}{2}}}\right)\right]. \qquad \ldots\ldots(26)$$

Thus, knowing τ, (26) may be solved for t, which value is used in (4) to give the moisture-content distribution.

In general the summation term in (26) is small compared with $\chi_3(0) = 0{\cdot}094$, increasing relatively for the larger times. In the particular cases encountered it did not exceed 4 % and could be neglected, whereupon (26) reduces to a cubic and can be solved in the usual manner. It was found more convenient, however, to construct a table giving τ in terms of t, and to obtain particular values by interpolation.

§ 4. EXPERIMENTAL CONFIRMATION

Confirmatory experimental evidence, similar to that described in the paper to which reference has already been made, was obtained by determining actual moisture-content distributions both in bars of uniform cross-section and in

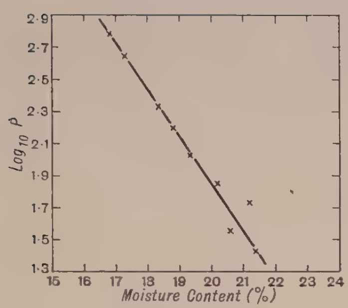

Figure 1.

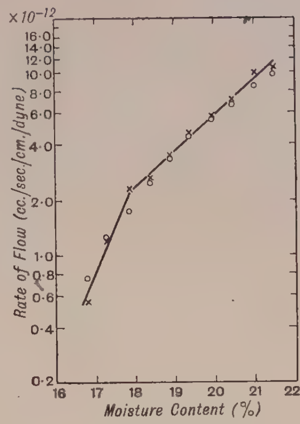

Figure 2.

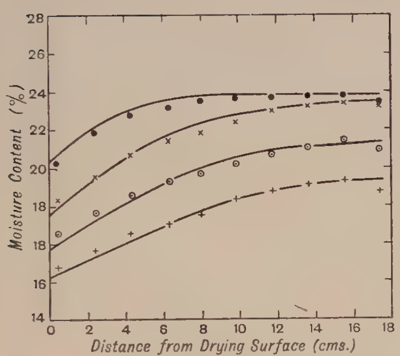

Figure 3.

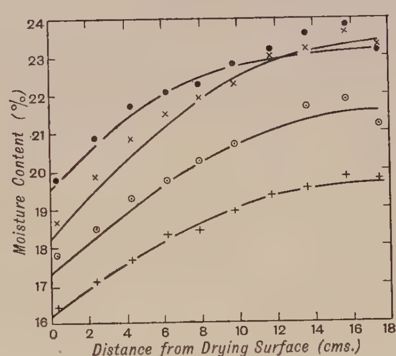

Figure 4.

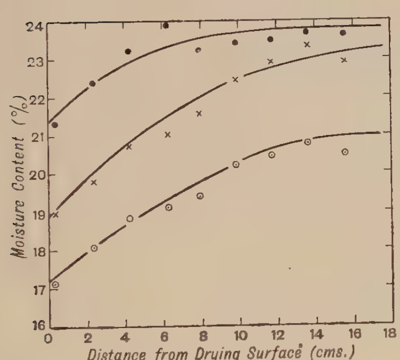

Figure 5.

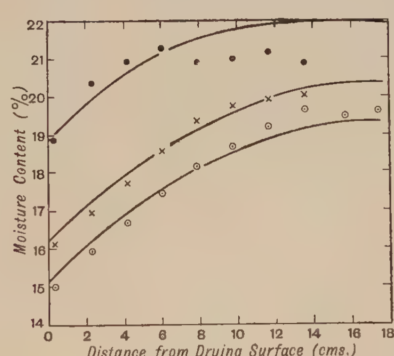

Figure 6.

Figure 1. Pressure-moisture content relation of clay B.

Figure 2. Conductivity of clay B.

Figures 3, 4. Comparison of experimental and calculated moisture-content distributions in the case of linear flow.

Figures 5, 6. Comparison of experimental and calculated moisture-content distributions in the case of radial flow.

wedge-shaped pieces, suitably covered to prevent drying except from their exposed ends. The experimental method of drying the testpieces under controlled conditions of temperature and humidity, and of cutting sections along their length after a noted time, calls for no particular comment. The clay used was a finely ground fireclay, known as Clay B, the relation between pressure and equilibrium moisture-content being given in figure 1, and the aqueous conductivity in figure 2. The characteristic constants of the clay are

$\alpha = 3{\cdot}422 \times 10^{12}$ $\qquad$ $S = 39{\cdot}8$
$\beta = 0{\cdot}6733$ $\qquad$ $M_L = 11{\cdot}5\%$
θ (above 17·8 %) $= 9{\cdot}116 \times 10^{-16}$ $\qquad$ $V_D = 51{\cdot}0$ c.c.
ϕ (above 17·8 %) $= 0{\cdot}4376$

In the above, M_L is the moisture-content at which shrinkage ceases (and below which the above theory is not valid) and V_D is the dry volume of 100 grams of the clay. From these and S, the volumetric shrinkage curve may be reconstructed.

Typical moisture-content distributions in bars are shown in figures 3 and 4, the rates of drying of those in figure 3 being, from top to bottom, 9·4, 8·5, 6·9 and $7{\cdot}25 \times 10^{-6}$ gm. sec.$^{-1}$ cm.$^{-2}$ respectively. The corresponding rates in figure 4 were 6·0, 6·5, 5·8, and $5{\cdot}7 \times 10^{-6}$ respectively. The points shown are the experimental values, and the curves were calculated by the method described above.

Moisture-content distributions in wedges are shown in figures 5 and 6, the rates of drying being: for figure 5, 5·1, 5·5 and $6{\cdot}7 \times 10^{-6}$ and for figure 6, 7·8, 10·1 and $10{\cdot}8 \times 10^{-6}$ gm. sec.$^{-1}$ cm.$^{-2}$ respectively. On cutting thin slices from the thinner end of the wedge, very small quantities of clay are available for moisture-content determination, and the experimental accuracy is naturally less in this case.

§ 5. THREE-DIMENSIONAL FLOW

It may be noted that a parabolic expression similar to (9) and (13) is a solution in the case of three-dimensional flow. On the same argument, (4) is also a solution on another different time scale.

§ 6. ACKNOWLEDGEMENTS

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ON SPACE-CHARGE EFFECTS IN VELOCITY-MODULATED ELECTRON BEAMS

By W. H. J. FUCHS and R. KOMPFNER

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ABSTRACT. For the purposes of analysis, a velocity-modulated beam of electrons is idealized in two different ways. The cross-section of the beam is assumed very large. In the first model the electrons move in a drift space enclosed by parallel planes having equal potential and screening the beam from the rest of space. The acceleration of an electron in the drift space is entirely due to space-charge forces. This mathematical model is used to discuss the working of velocity-modulated tubes as high-frequency amplifiers. The main conclusion is that the beam of electrons operates in the same manner as if space charge were absent, except that the mean transit-time is altered. The saturation current-density obeys a $V^{3/2}$ law in its dependence on the accelerating voltage V. For given j_0 and V, the length of the tube must be below a critical value in order that a continuous beam of electrons can exist. The limiting length of an electron beam (subject to the above assumptions) is just under three times the plate separation of a saturated plane diode passing the same current per cm^2.

The analysis further shows the existence of a potential hill with its crest about half-way across the drift space. Electrons entering the space are slowed down by the space charge in front of them until they reach the summit of the potential hill; from there on they are accelerated again by the space charge behind them.

The second model beam, called the *domain* model, if anything under-estimates the effects of space charge. A domain may be thought of as a thin slice of beam which moves under the influence of its own initial distribution of charge and velocity, the effects of neighbouring domains and of the boundaries being neglected. In the course of the motion of a domain, two kinds of charge-density maxima will occur. The first kind ("bunching") occurs when and where successive layers of electrons overtake one another. The second kind ("compression") occurs while no overtaking takes place. Successive layers of electrons approach each other to a minimum distance and then move further apart again. The conditions for bunching are found.

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§ 1. INTRODUCTION

THE purpose of this paper is the discussion of two mathematical models of velocity-modulated beams of electrons. The study of such beams has recently attracted considerable attention. D. L. Webster (1939) has given a complete mathematical analysis under the assumptions that the modulation is small and sinusoidal and that the influence of space charge may be neglected. His paper also contains a calculation concerning the effect of space charge. This calculation is based on several assumptions, some of them

rather remote from physical reality. In particular it is suggested that Webster's assumption that the average, or unmodulated, value of space-charge density is zero (as if cancelled by positive ions) prevents a proper estimate of the importance of space charge being made. It may be of interest to discuss the effect of space charge under different assumptions. We have constructed two different mathematical models of velocity-modulated beams.

We apply the first model, which is discussed in § 2, to the study of drift tubes working as amplifiers at very high frequencies. The second model, described in § 3, is devised so as to allow a very simple analysis of the process of "bunching" in the presence of space charge.

Throughout the paper we make the following assumptions:—

(i) The beam is of infinite cross-section and all electrons move along parallel lines, so that the problem becomes one-dimensional.

(ii) Electro-magnetic and relativistic effects can be neglected. This assumption is justified except for very fast electrons.

§ 2. DISCUSSION OF THE FIRST MODEL

2.1. *Assumptions and notation.* In this section we make the following additional assumptions:—

(*a*) We consider the motion of electrons between two infinite planes $x=0$ and $x=l$. The electrons enter the space between the planes through $x=0$ with the velocity $v_0(t)$ and fly across it to $x=l$ along straight lines parallel to the x-axis. The drift-space between $x=0$ and $x=l$ is electrostatically screened against the rest of space.

(*b*) The current-density at $x=0$ is constant and equal to $-j_0$ in the direction of increasing x.

(*c*) There is no overtaking of electrons.

(*d*) $v_0(t)=v_0+v_1(t)$, where v_0 is constant and $v_1(t)$ is a periodic modulation. The mean value of $v_1(t)$ over a period is 0.

(*e*) The mean transit time of electrons through the space we call T. We write $h(t)=T(1+\eta(t))$ for the time of transit of an electron which leaves the space at the time t. Because of (*d*), $\eta(t)$ is periodic, and by the definition of T the mean value of $\eta(t)$ over a period is 0. From 2.3 on, we shall assume that $\eta(t)$ is small.

For convenience we give the following list of our symbols:—

Time: t.

Charge of electron: $-\boldsymbol{e}$. Mass of electron: $\boldsymbol{m}$.

Charge-density: $-\rho(x, t)$.

Velocity of electron which entered the space at $t=t_1$: $v(t, t_1)$.

Distance traversed by this electron: $x(t, t_1)$.

Time of transit through the space of the electron passing $x=l$ at the moment t: $h(t)$.

Current-density at $x=0$: $-j_0$.

Velocity of electron at $x=0$: $v_0(t)=v_0+v_1(t)$.

$$\beta=2\pi(e/m)j_0.$$

Electrostatic and c.g.s. units are employed throughout the calculation, unless stated otherwise.

2.2. *The equation of motion.* The force exerted on an electron by the space charge between the planes x and $x+dx$ is the same as that due to a condenser plate carrying a charge $-\rho(x, t)dx$ per unit area. This force is $2\pi e\rho(x, t)dx$, repelling the electron from the plate. Hence the total force on the electron at $x=u$ which entered the space at $t=t_1$, say, is

$$m\frac{dv(t, t_1)}{dt}=2\pi e\left\{\int_0^u \rho(x, t)dx-\int_u^l \rho(x, t)dx\right\}. \qquad \ldots\ldots(1)$$

This equation can be written in a different form, if we introduce the transit-time $h(t)$ of an electron. The first integral in (1) is, apart from sign, the charge per unit cross-section to the left of the electron (with the usual orientation of the x-axis). That is to say, it is the charge which entered the space between the time t_1 and the time t. But this charge is equal to $-j_0(t-t_1)$ by assumption (b). Hence

$$\int_0^u \rho(x, t)dx=j_0(t-t_1).$$

Similarly

$$\int_u^l \rho(x, t)dx=j_0(t_1-t+h(t)) \text{ (see figure 1)}.$$

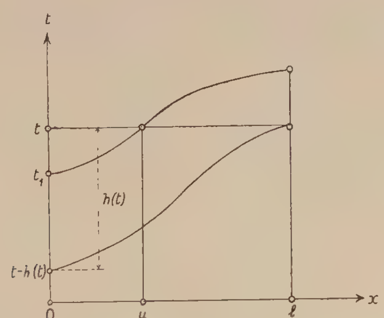

Figure 1. Space-time diagram of electron motion.

Therefore we can re-write (1) as

$$\frac{dv(t, t_1)}{dt}=\beta\{2(t-t_1)-h(t)\}, \qquad \ldots\ldots(1a)$$

where $\beta=2\pi(e/m)j_0$. Two successive integrations give

$$x(t, t_1)=\beta\left\{\tfrac{1}{3}(t-t_1)^3-\int_{t_1}^t du\int_{t_1}^u h(v)dv\right\}+v_0(t_1)(t-t_1). \qquad \ldots\ldots(2)$$

If $x(t, t_1) = l$, we can write $t - h(t)$ instead of t_1 by the definition of $h(t)$. After a simple transformation (2) now becomes

$$l = \beta\left\{\tfrac{1}{3}h^3(t) - \int_0^{h(t)} zh(t-z)dz\right\} + v_0(t - h(t)) \, . \, h(t). \qquad \ldots\ldots(3)$$

If $h(t)$ has been found from (3), the motion is completely determined. If, for instance, we want to find the current-density at $x = l$, we need only remember that the charge passing through $x = l$ per unit area in the time interval between t and $t + \delta t$ is the charge which entered the space between the time $t - h(t)$ and $t + \delta t - h(t + \delta t)$, so that the current-density at $x = l$ is

$$\lim_{\delta t \to 0}\left[-j_0 \frac{\delta t - h(t+\delta t) + h(t)}{\delta t}\right] = -j_0(1 - h'(t)). \qquad \ldots\ldots(4)$$

2.3. *Simplification of the equation of motion for small $\eta(t)$*

We assume from now on that the variable part $T\eta(t)$ of $h(t)$ is small compared with the mean transit time T. This will be the case if, and only if, the modulation

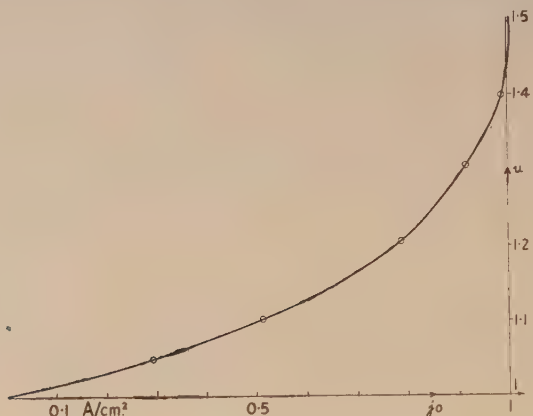

Figure 2. $j_0 - u$.

j_0	0·287	0·51	0·79	0·93	0·994	1·004
u	1·05	1·1	1·2	1·3	1·4	1·5

$v_1(t)$ is small compared with v_0. We shall from now on neglect terms of the order of $\eta^2(t)$ and the product $v_1(t)\eta(t)$ in comparison with terms of the order of unity. Writing $h(t) = T[1 + \eta(t)]$, and omitting small terms, we obtain from (3)

$$\begin{aligned} l &= \beta\{\tfrac{1}{3}T^3 + T^3\eta(t) - \tfrac{1}{2}T^3 - T^3\eta(t) - T\int_0^T z\eta(t-z)dz\} \\ &\quad + v_0T + v_0T\eta(t) + Tv_1[t - h(t)] \\ &= \beta\{-\tfrac{1}{6}T^3 - T\int_0^T z\eta(t-z)dz\} + v_0T + v_0T\eta(t) + Tv_1\{t - h(t)\}. \end{aligned} \qquad \ldots\ldots(5)$$

We split up (5) into the two equations,

$$l = -\tfrac{1}{6}\beta T^3 + v_0 T \qquad \ldots\ldots(6)$$

and

$$0 = v_0 T\eta(t) + Tv_1[t - h(t)] - \beta T\int_0^T z\eta(t-z)dz. \qquad \ldots\ldots(7)$$

(6) is an equation for the mean transit time T. It shows that the presence of space charge increases T above its value $T' = l/v_0$, which holds when there is space charge.

For the ratio $T/T' = u$ we have the equation

$$-\tfrac{1}{3}au^3 + u = 1, \qquad \ldots\ldots(8)$$

where a is the dimensionless number $\frac{1}{2}\beta l^2 v_0^{-3}$. The left side of (8) has a maximum at $u = a^{-\frac{1}{2}}$ where it is equal to $\frac{2}{3}a^{-\frac{1}{2}}$. Therefore (8) has two positive roots as long as $\frac{2}{3}a^{-\frac{1}{2}} > 1$, i.e. $a < 4/9$. Only the smaller of these roots has physical significance. As a increases from 0 to its largest possible value 4/9, the influence of space charge becomes more and more appreciable until u is increased from its " no-space-charge " value 1 to 1·5. If a is increased beyond 4/9, the equation (8) no longer has positive roots and an electron beam cannot be formed. We have, therefore, the following condition for the existence of a continuous beam:—

$$4/9 \geqslant a = \pi(e/m)j_0 l^2 v_0^{-3} \quad \text{or} \quad l \leqslant \tfrac{2}{3}(2v_0^3/\beta)^{\frac{1}{2}} \quad \text{or} \quad j_0 \leqslant m v_0^3 l^{-2}/9\pi e.$$

Again,

$$T' < T \leqslant \tfrac{3}{2}T' \leqslant \left(\frac{2v_0}{\beta}\right)^{\frac{1}{2}}. \qquad \ldots\ldots(9)$$

If v_0 is expressed in terms of the accelerating voltage $V_0 (v_0 = (2eV_0/m)^{\frac{1}{2}})$, the critical length and current are given by

$$l = \left\{\frac{8\sqrt{(2e/m)}}{9\pi j_0}V_0^{3/2}\right\}^{\frac{1}{2}}, \quad j_0 = \frac{8\sqrt{(2e/m)}}{9\pi l^2}V_0^{3/2}, \qquad \ldots\ldots(9')$$

so that we have a $V^{3/2}$ law. It is interesting to compare (9′) with Langmuir's formula,

$$L = \left\{\frac{\sqrt{(2e/m)}}{9\pi I}V^{3/2}\right\}^{\frac{1}{2}},$$

connecting the plate distance L, the saturation current I per cm^2 and the applied voltage V of a plane diode. If $V = V_0$,

$$l/L = 2\sqrt{(2I/j_0)}.$$

In particular, the critical length of an electron beam is $2\sqrt{2}$ times the plate separation of a Langmuir diode passing the same saturation current at the same accelerating voltage.

It may be instructive to examine in more detail the manner in which the space charge increases the transit time. We confine ourselves to the case of a steady, unmodulated current. This special case shows all the relevant features.

For a steady current $v_0(t)=v_0$ and $h(t)=T$. Using these values in (1a) with $t_1=0$ (no loss of generality) and integrating twice we obtain

$$v(t)=v(t,0)=\beta\{t^2-Tt\}+v_0,$$
$$x(t)=x(t,0)=\beta\{\tfrac{1}{3}t^3-\tfrac{1}{2}Tt^2\}+v_0t.$$

The potential $-V$ at any point is given by the equation of energy,

$$\tfrac{1}{2}\boldsymbol{m}\boldsymbol{v}^2+\boldsymbol{e}\boldsymbol{V}=\tfrac{1}{2}\boldsymbol{m}\boldsymbol{v}_0{}^2.$$

The function $v(t)$ decreases from v_0 to a minimum at $t=\tfrac{1}{2}T$, i.e. half-way across the drift space. From there on, the velocity increases again until it reaches its original value v_0 at $x=l$. Correspondingly there is a " potential hill " with its crest at the middle of the drift space. Electrons are slowed down while going up the hill and gain speed while going down on the other side. In the critical case, the reduction of speed at the crest of the hill is as much as 50 %.

Numerical example:—Suppose $l=2$ cm., $v_0=3600$ volts. Then the critical current-density $j_0=1{\cdot}004$ amp./cm., minimum velocity = 900 v. The increase of u with j_0 is shown in figure 2.

2.4. *Discussion of a special case.* In general it will not be possible to find $\eta(t)$ explicitly from (7). We restrict ourselves further in discussion to the special case where

$$v_1(t)=\tfrac{1}{2}Mv_0\sin\omega t.$$

By our previous assumptions, M must be small. This will certainly be the case for a drift tube working as an amplifier, where we may reasonably suppose that M is of the order of 10^{-2}. We are mainly interested in oscillations of very high frequency, for which we may assume that the mean transit time T is not much below the time of two complete cycles. The period of a cycle is $2\pi/\omega$, so that we shall have $T(2\pi/\omega)^{-1}$ not much below 2, or, say,

$$\omega T\geqslant 10.$$

We may assume that j_0 is of the order of $0{\cdot}1$ ma. This value is probably below those employed in practice, but, on the other hand, our assumption of an infinite cross-section over-estimates the influence of space charge very considerably.. With $j_0=0{\cdot}1$ ma. we should have

$$\beta=9.10^{23}\text{ cm./sec.}^{-3},$$

approximately. For v_0 and ω it will be reasonable to assume 2.10^9 and 10^9 respectively.

We assume for the moment that it will be possible to find a first approximation to $\eta(t)$ by neglecting the integral in (7) altogether. We shall see afterwards whether this assumption is justified. (7) is now reduced to

$$\eta(t)=-\tfrac{1}{2}M\sin\omega(t-T-T\eta(t)). \qquad \ldots\ldots(7a)$$

In this equation β no longer occurs explicitly, and it is easy to see that (7a) is valid in the case of no space charge, if we take for T the value $T'=l/v_0$. We can find $\eta(t)$ explicitly by applying the following special case of

Lagrange's theorem. If $y=r\phi(y)$, $\phi(0)=0$, then

$$y=\sum_{n=1}^{\infty}\frac{r^n}{n!}\left(\frac{d}{dy}\right)^{n-1}[\phi(y)]^n_{y=0}.$$

With

$$y=\omega T\eta(t),\quad x=\omega(t-T),\quad r=-\tfrac{1}{2}\omega TM,\ \phi(y)=\sin(x-y)$$

we obtain, after some re-arrangements,

$$\eta(t)=-\frac{2}{\omega T}\sum_{m=1}^{\infty}\frac{1}{m}J_m(mr)\sin mx, \qquad \ldots\ldots(10)$$

where $J_m(x)$ is the Bessel function of order m.*

We must now investigate whether this solution represents a sufficiently good approximation. With the numerical values of β and v_0 given above we have, by (9),

$$T<6{\cdot}7\,.\,10^{-8}\,\text{sec.} \qquad \ldots\ldots(11)$$

Hence $|r|=\frac{1}{2}M\omega T<0{\cdot}5$, say. For $|r|<\frac{1}{2}$, a fair approximation to the right-hand side of (10) is

$$\eta(t)=-\tfrac{1}{2}M\sin x. \qquad \ldots\ldots(12)$$

If we use this expression for $\eta(t)$ we find

$$\int_0^T z\eta(t-z)dz=-\tfrac{1}{2}(MT/\omega)\cos(x-\omega T)-\tfrac{1}{2}M\omega^{-2}(\sin(x-\omega T)-\sin x),$$

so that the integral in (7) is indeed small compared with the first two terms, since

$$\beta T/\omega v_0<3\,.\,10^{-2};\quad \beta/v_0\omega^2<5\,.\,10^{-4}.$$

The approximation (12) is, therefore, quite satisfactory, as its deviation from the true value of $\eta(t)$ will be well below 10 %.

By (4) and (12), the current density at $x=l$ is given by

$$j(l)=j_0(1+\tfrac{1}{2}M\omega T\cos\omega(t-T)). \qquad \ldots\ldots(13)$$

2.5. *Conclusion.* If we suppose the velocity of the electrons due to an applied voltage U modulated by a signal $MU\sin\omega t$, then (13) shows that the beam-current magnification-factor is $\frac{1}{2}\omega T$. This formula is also true in the case of no space charge, if $T=T'=l/v_0$. In this case the factor can be made arbitrarily large by choosing l large. If the space charge is taken into consideration, the magnification factor is limited by (9). With our numerical values, the maximum is about 30, which would be attained in a drift tube of about 80 cm. We do not want to lay stress on this numerical result, however.

The main point of our calculation is that *below a certain critical length, velocity-modulated beams of electrons can operate in the same manner as beams of velocity-modulated particles on which no space charge is acting. The chief effect of space charge is an increase in the mean transit time. Formulae valid in the case*

* This result also follows from Webster's work, loc. cit.

of no space charge represent good first approximations if $T' = l/v_0$ is replaced by the actual mean transit time T. Above the critical length, the space charge prevents the formation of a continuous beam.

When the current density is high, β is very large and the maximum attainable value of T as calculated from (9) becomes very small. On the other hand, the beam-current magnification is proportional to T, and it is therefore desirable to give T a large value. This cannot be done by a mere lengthening of the tube, because of the space charge. It seems, therefore, advantageous to reduce the effect of space charge. The easiest way to do this would be to leave some gas in the drift tube, which would yield positive ions under electron bombardment.

In the opinion of the authors, the qualitative conclusions reached are also valid for beams of finite cross-section. This opinion is supported by the following calculation, which establishes the existence of a " potential hill " for an (unmodulated) electron beam of cylindrical cross-section. In such a beam we shall therefore have a slowing up of the electrons and a consequent increase of the mean transit-time T.

Suppose this were not the case. Then electrons would move parallel to the axis of the beam at a nearly constant velocity v_0 and the beam would represent a cylinder of space charge of circular cross-section, radius a, say, and of length l (we neglect the gradual broadening of the beam due to space charge). The density of the space charge would be $-\rho = -j_0/v_0$, uniform throughout the cylinder. Let V be the potential at $x=0$, V_1 the potential at $x=\frac{1}{2}l$, half-way along the beam, both measured on the axis of the beam. Let v_1 be the velocity of an electron at $x=\frac{1}{2}l$. By our assumption, v_0 and v_1 are practically equal. On the other hand, an elementary integration shows that

$$\begin{aligned}
\tfrac{1}{2}\boldsymbol{m}(v_0{}^2 - v_1{}^2) &= -\boldsymbol{e}(V_1 - V_0)\\
&= 2\pi\rho\boldsymbol{e}\int_{-k}^{k} du \int_0^a \left[\frac{r\,dr}{\{r^2+u^2\}^{\frac{1}{2}}} - \frac{r\,dr}{\{r^2+(k-u)^2\}^{\frac{1}{2}}}\right]\\
&= 2\pi\rho\boldsymbol{e}\{k^2 + k(a^2+k^2)^{\frac{1}{2}} - k(a^2+4k^2)^{\frac{1}{2}}\\
&\qquad + \tfrac{1}{2}a^2\log[k+(a^2+k^2)^{\frac{1}{2}}] + \tfrac{1}{2}a^2\log[-2k+(a^2+4k^2)^{\frac{1}{2}}]\\
&\qquad - \tfrac{1}{2}a^2\log[-k+(a^2+k^2)^{\frac{1}{2}}] - \tfrac{1}{2}a^2\log a\},
\end{aligned}$$

where $k=\frac{1}{2}l$. If a is small compared with k, this is approximately equal to

$$\left.\begin{aligned}
\tfrac{1}{2}\boldsymbol{m}(v_0{}^2 - v_1{}^2) &= \pi\boldsymbol{e}\rho a^2(\tfrac{1}{2} + \log l/2a),\\
1-(v_1/v_0)^2 &= 2\pi(\boldsymbol{e}/\boldsymbol{m})\rho a^2 v_0{}^{-2}(\tfrac{1}{2} + \log l/2a).
\end{aligned}\right\} \quad \ldots\ldots(14)$$

If our assumption is justified, both sides of (14) are small. The assumption ceases to be justified if

$$2\pi(\boldsymbol{e}/\boldsymbol{m})\rho a^2 v_0{}^{-2} = 2\pi(\boldsymbol{e}/\boldsymbol{m})j_0 a^2 v_0{}^{-3} = 2(\boldsymbol{e}/\boldsymbol{m})i\,v_0{}^{-3}$$

is not small. Here $i=\pi j_0 a^2$ is the total current.

§ 3. THE DOMAIN MODEL

3.1. In this section we think of a beam as split up into several parts. One such part we call a *domain*.

The treatment of our problem is then simplified by the following *assumptions*:—

(*a*) We consider the behaviour of a domain under the influence of its own initial distribution of charge and velocity only. All external influences, e.g. of neighbouring domains or of the boundaries, are neglected.

(*b*) We assume that initially the domain is bounded by two perpendicular cross-sections of the beam $2A$ apart. Charge density and velocity distribution at the time $t=0$ are given by

$$-\rho=-\rho_0(a),$$
$$v=v_0+w_0(a);\quad w_0(0)=0.$$

Here a is the distance (positive or negative) from the centre of the domain. We shall imagine that the modulation $w_0(a)$ of the mean velocity v_0 of the electrons is due to their just having passed through a modulating grid at the time $t=0$. We assume that before their passage through the grid they moved with uniform velocity v_0, so that the beam represented a steady current of density j. At the time $t=0$, the charge density of the domain will no longer be uniform, because an interval of time has elapsed since the first electrons passed the modulating grid. During this time the modulation of velocity has altered the relative position of the electrons of the domain. As a first approximation we shall, however, assume that

$$\rho_0(a)=\rho_0=j/v_0.$$

The domain has passed the modulating grid in $2A/v_0$ seconds. Later we shall assume that the modulation is periodic with frequency f, and we shall choose A so that $2A/v_0$ is equal to a complete cycle of the modulation, i.e. $2A/v_0=1/f$.

(*c*) There is no mutual overtaking of electrons.

3.2. *Equation of motion.* To simplify the analysis of the model, we subtract the common translational velocity v_0 from the absolute velocity v of an electron, so that we are only studying the distribution of relative velocities $w=v-v_0$. This is equivalent to assuming that the observer is moving along the beam with velocity v_0.

As in § 2, we regard the space charge of the domain as a collection of thin condenser plates. Each one of these exerts a force proportional to its charge density on an electron. The total force on an electron at a distance a from the centre is, therefore, by the reasoning of § 2,

$$m\frac{dv}{dt}=m\frac{dw}{dt}=2\pi e\left\{\int_{-A}^{a}\rho\,du-\int_{a}^{A}\rho\,du\right\}. \quad \text{......(15)}$$

Because of assumption (*c*), the right-hand side of (15), which represents $-2\pi e$

times the difference of charge per unit cross-section on both sides of the electron, is constant throughout the motion for a given electron.

When the initial distribution is uniform, (15) becomes

$$\frac{dw}{dt} = \frac{dw(t,a)}{dt} = 2\pi(\boldsymbol{e}/\boldsymbol{m})\rho_0 \,.\, 2a \qquad \ldots\ldots(16)$$

for an electron which is at a distance a from the centre at the time $t=0$. Integrating, we obtain

$$w(t,a) = 2\rho at + w_0(a),$$

where $\sigma = 2\pi(\boldsymbol{e}/\boldsymbol{m})\rho_0$. Let an electron originally at a be at a distance $z(t,a)$ from the centre of the domain at the time t. One further integration gives us

$$z(t,a) = a(\sigma t^2 + 1) + w_0(a)t. \qquad \ldots\ldots(17)$$

3.3. *Charge-density maxima.* By (17) the motion of the domain is completely determined. From the principle of conservation of charge, we deduce the charge density at z at the time t by means of the relation

$$\rho(z,t)\Delta z = \rho_0 \Delta a,$$

which shows that

$$\rho(z,t) = \rho_0(\partial z/\partial a)^{-1}. \qquad \ldots\ldots(18)$$

We investigate in particular the occurrence of maxima of charge density. We have to distinguish between two kinds of such maxima. The first kind is characterized by the fact that it occurs when and where overtaking of electrons occurs. For this kind we propose to retain the term *bunching*. The second kind of maximum is characterized by the fact that it occurs where electrons do not overtake each other; such a maximum is closely analogous to a region of maximum density of a longitudinally vibrating elastic rod. This type of maximum we propose to call a *compression*. The two types are illustrated in the space-time diagrams (figures 3 *a* to 3 *c*). The charge density between z and $z+\Delta z$ at the time t_1 is proportional to the number of space-time lines crossing the line $t=t_1$ between z and $z+\Delta z$.

We shall now write down the conditions for bunching and for compression. Bunching occurs when two successive layers of electrons coincide, that is to say, the co-ordinate z of two infinitesimally close electrons becomes equal. The mathematical expression for this is

$$\frac{\partial z(t_b, a)}{\partial a} = 0. \qquad \ldots\ldots(19)$$

For a given value of a, we can find the time of bunching t_b from this equation. We must bear in mind, however, that only the minimum of t_b for varying a, t_b', say, has physical significance. t_b' is the time at which bunching first occurs. For $t > t_b'$ assumption (*c*) is no longer satisfied, and, therefore, our equations no longer hold. To find t_b' we notice that

$$\left(\frac{dt_b}{da}\right)_{t=t_b'} = 0.$$

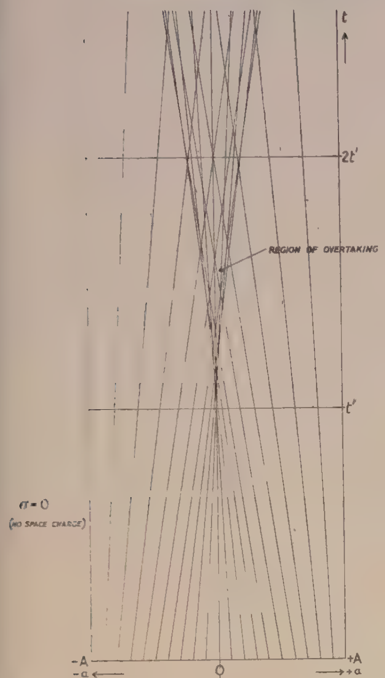

Figure 3 (*a*). Space-time diagram illustrating the no-space-charge case.

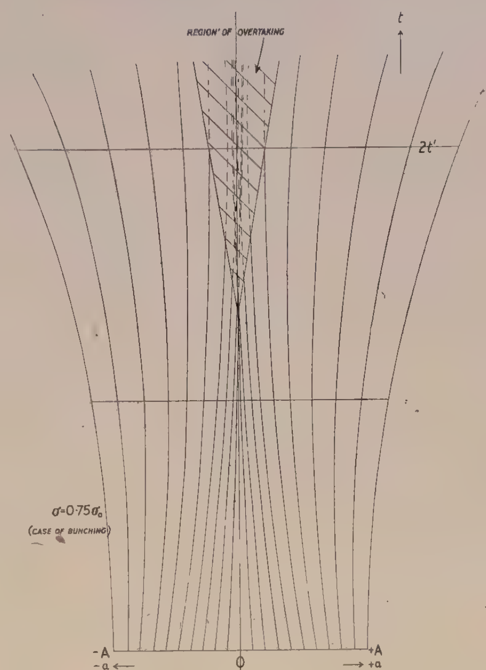

Figure 3 (*b*). Space-time diagram illustrating the case when the space-charge density is less than the critical.

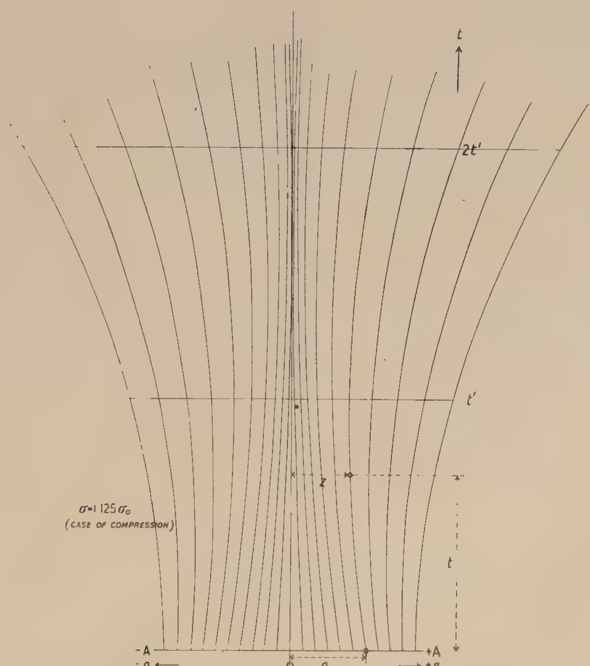

Figure 3 (*c*). Space-time diagram showing the case when the space-charge density is greater than the critical.

Differentiating (19) with respect to a we have

$$\frac{\partial^2 z}{\partial a^2} + \frac{dt_b}{da}\frac{\partial^2 z}{\partial a\,\partial t} = 0,$$

and, therefore, for $t_b = t_b'$,

$$\partial^2 z/\partial a^2 = 0. \qquad \ldots\ldots(20)$$

This is also the condition for " optimum bunching ", in the sense that electrons originally filling an interval Δa are compressed into an interval of order less than $(\Delta a)^2$ at the point of bunching. Collecting all our information, we have the following conditions for bunching:

$$\frac{\partial z}{\partial a} = 0; \quad \frac{\partial^2 z}{\partial a^2} = 0. \qquad \ldots\ldots(21)$$

We find now the condition for compression. Electrons originally between the planes $a + \Delta a$ and a are compressed after a time t into a layer of thickness Δz. To the first order of magnitude of small quantities

$$\Delta z = \frac{\partial z}{\partial a}\Delta a. \qquad \ldots\ldots(22)$$

Necessary conditions that Δz should be a minimum are

$$\partial \Delta z/\partial a = 0, \quad \partial \Delta z/\partial t = 0,$$

or, using (22),

$$\partial^2 z/\partial a^2 = 0, \quad \partial^2 z/\partial t\,\partial a = 0. \qquad \ldots\ldots(23)$$

3.4. *Choice of $w_0(a)$.* We shall now use our results to discuss two special cases obtained by putting

$$\left.\begin{array}{l} \text{(i)}\ \ w_0(a) = u(a) = v_0\{(1 - M\sin(\pi a/A))^{\frac{1}{2}} - 1\}, \\ \text{(ii)}\ \ w_0(a) = y(a) = v_0\{(1 + M\sin(\pi a/A))^{-\frac{1}{2}} - 1\}. \end{array}\right\} \qquad \ldots\ldots(24)$$

$u(a)$ is the velocity distribution generated by a sinusoidal modulating voltage. $y(a)$ has the advantage of leading to expressions identical with those given in a previous paper by one of the authors (Kompfner, 1940). Both distributions are approximately equal to $-\frac{1}{2}v_0 M \sin(\pi a/A)$ for small values of M.

Neither $u(a)$ nor $y(a)$ has an exact counterpart in reality. But it is evident that discrepancy between assumed and practically realizable distributions becomes noticeable only for values of M near unity. For values of M up to 0·5, say, we can be assured of a high degree of similarity to a real velocity distribution.

3.5. *The case of no space-charge.* Before applying our formulae to the velocity distributions (24) it will be useful to discuss the motion disregarding space-charge. In this case (17) reduces to

$$z(t, a) = a + w_0(a). \qquad \ldots(25)$$

The condition for bunching is still given by (21). Compression cannot occur in this case. From (21) and (25) we have for the time of optimum bunching

$$t' = -(dw_0/da)^{-1} \text{ when } d^2w/da^2 = 0.$$

A simple calculation shows that in case (i)

$$t' = (\pi f M)^{-1} C_1, \qquad \ldots\ldots(26)$$

where

$$C_1 = M[2\{1 - \sqrt{(1 - M^2)}\}]^{-}.$$

In case (ii) we have

$$t' = (\pi f M)^{-1} C_2, \qquad \ldots\ldots(27)$$

where

$$C_2 = \frac{1}{\sqrt{2}}[2 - \sqrt{(1+3M^2)}]\,[(1 + \sqrt{(1+3M^2)}]^{-\frac{1}{2}}.$$

In both cases we have used the relation $2A/v_0 = 1/f$.

The constant C_2 is identical with the "correction-factor" c of the paper cited above. For small M, C_1 and C_2 are very nearly unity.

3.6. *Times of optimum bunching and compression.* We can now proceed to

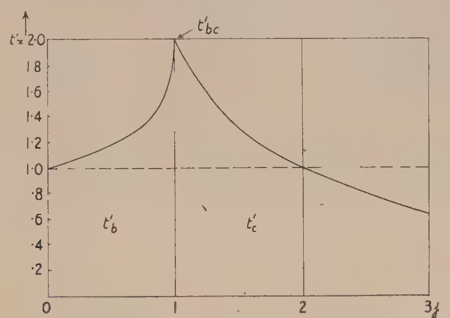

Figure 4. Time of optimum bunching and compression as function of the beam current j.

calculate the time of optimum bunching in the case of space-charge. By (17) we can write the condition for (optimum) bunching in the form

$$\frac{\partial z}{\partial a} = \sigma t^2 + 1 + \frac{dw_0(a)}{da}\,.\,t = 0 \qquad \ldots\ldots(28)$$

and

$$d^2 w_0 / da^2 = 0. \qquad \ldots\ldots(29)$$

From (29) we find a. Since this equation is independent of σ and t, the solution yields the same value as in the case of no space-charge, and, therefore, substituting for a in (28) $\sigma t^2 + 1 - t/t' = 0$,

$$t_b' = \frac{1 - \sqrt{(1 - 4\sigma t'^2)}}{2\sigma t'}. \qquad \ldots\ldots(30)$$

The value of t' is given either by (26) or by (27), according to the choice of $w_0(a)$.

In the case of compression we have similarly

$$\frac{\partial^2 z}{\partial t\,\partial a} = 2\sigma t + \frac{dw_0}{da} = 2\sigma t - t'^{-1} = 0,$$

so that

$$t_c' = 1/2\sigma t'. \qquad \ldots\ldots(31)$$

If $4\sigma t'^2 < 1$, t_b' is real, and bunching occurs:—If $4\sigma t'^2 > 1$ the expression (30) is complex, bunching cannot occur and we shall have compression instead. For

$$\sigma = \sigma_0 = \tfrac{1}{4} t'^{-2}$$

we have a border-line case between bunching and compression; some electrons catch up with those immediately in front of them, but they have not sufficient speed to overtake them. The time at which this catching up occurs is given by using this value of σ in (31); thus,

$$t_{bc}' = 2t'.$$

If $\sigma = \sigma_0$, we find from the equations

$$\sigma = 2\pi(\boldsymbol{e}/\boldsymbol{m})\rho, \quad j = \rho v_0,$$

that the current-density j is given by

$$j = \frac{v_0}{2\pi(\boldsymbol{e}/\boldsymbol{m}) \,.\, 4t'^2},$$

or, using (26) and (27),

$$j = \frac{\pi v_0 M^2 f^2}{8(\boldsymbol{e}/\boldsymbol{m}) C_{1,2}^2} = 1{\cdot}46 \,.\, 10^{-20} \sqrt{V_0} f^2 M^2 C_{1,2}^{-2} \text{ amp./cm.}^{-2}.$$

where V_0 is the initial accelerating potential difference in volts.*

For current densities below the critical density j we have bunching proper, while at current densities above j we have compression only.

3.7. *Compression factor*. By the term *definition* we understand the minimum thickness Δz into which a given portion of initial thickness Δa, called the *aperture*, can be compressed. The ratio aperture/definition $= \Delta a/\Delta z$ is called the *compression factor*. In the case of the chopped beam particularly, but also quite generally, it will be of interest to investigate how the compression factor depends on the aperture and the choice of various other factors.

We write $\Delta w_0(a)$ for $w_0(a + \Delta a)$. By a simple calculation we find from (17) that the minimum Δz is

$$\Delta a \left[1 - \frac{1}{4\sigma}\left(\frac{\Delta w_0}{\Delta a}\right)^2\right],$$

and that it is attained at the time

$$t_m = -\frac{1}{2\sigma} \frac{\Delta w_0}{\Delta a}.$$

In the case of compression, this formula is true without restriction. In the case of bunching it is only valid if $t < t_b'$, since after that time our formulae are no longer applicable.

If we restrict ourselves to small M with

$$w_0(a) = -\tfrac{1}{2} v_0 M \sin(\pi a/A),$$

and to apertures symmetrical about $a = 0$, the compression factor becomes

$$\frac{\Delta a}{\Delta z} = \left[1 - \frac{1}{4\sigma}\left(\frac{\pi v_0 M \sin \Delta x}{2A\Delta x}\right)^2\right]^{-1},$$

* Thus in a practical example where $V_0 = 3600$ volts, $\lambda = 8$ cm. ($f = 3{\cdot}75 \times 10^9$ cycles/sec.) and $M = 1/\pi$, the critical current j, above which no proper bunching would be possible, will be $1{\cdot}4$ amp./cm.$^{-2}$. For $\lambda = 1$ cm., j will be of the order of 100 amp./cm.$^{-2}$.

where $\Delta x = \pi\Delta a/2A$. If $2A = v_0/f$, $\pi fM = t'^{-1}$, $\sigma_0 = \frac{1}{4}t'^{-2}$,

$$\frac{\Delta a}{\Delta z} = \left[1 - \frac{\sigma_0}{\sigma}\left(\frac{\sin \Delta x}{\Delta x}\right)^2\right]^{-1}.$$

It can be seen from this equation and from the graph (figure 5) that the quality of the compression deteriorates rapidly as the current density increases, while the time of optimum compression diminishes.

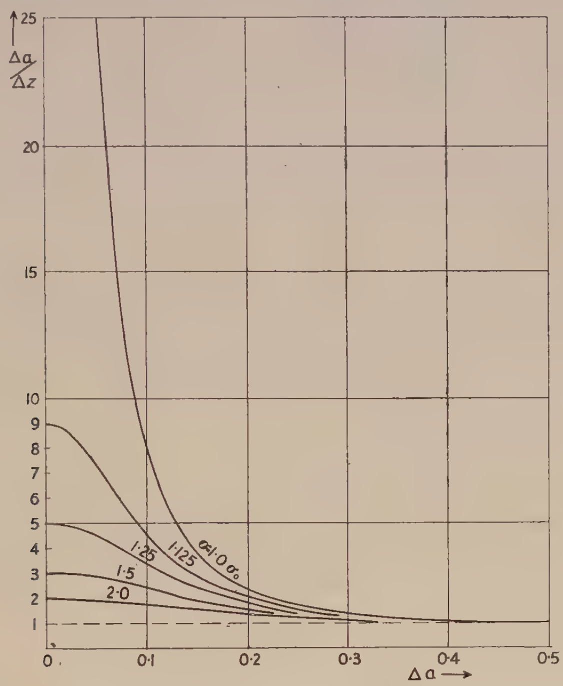

Figure 5. Compression factor $\Delta a/\Delta z$ as function of aperture Δa ($M \ll 1$).

3.8. *Validity of results.* The domain model has the obvious disadvantage that its assumptions are highly artificial. Even so, we believe that at least the following qualitative result of this section is correct:—

There will be overtaking and proper bunching with attendant split-bunching when the current density is below a certain critical value. Above this critical value there will be no proper bunching, the mutual repulsion of electrons will prevent any overtaking. If the current density is taken too high, the current peaks will become low and flat.

We can even argue that the numerical results of this section have some practical significance for the following two reasons:—

1. The error introduced by assuming the cross-section of the beam as infinite will be relatively small if the domain is not too wide. It may also be partially offset by the total neglect of the space-charge outside the domain.

2. In the middle of the domain, the effects of the space-charge on both sides outside the domain will probably cancel each other. To a first approximation, the effect of the neglected space-charge will be a compression of the domain, preventing the gradual broadening which takes place in our model.

If these two assumptions are granted, it follows, for example, that the formulae for the time of optimum bunching and compression must have a fairly close relation to reality.

Advantages of the domain model are its great simplicity, and the fact that its application does not in any way presuppose that the modulation is small and sinusoidal.

§ 4. ACKNOWLEDGEMENT

We wish to thank Dr. G. Liebmann for much helpful criticism.

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Note added 20. 12. 41. At the time of writing of this paper we had no access to scientific literature; we have found since that the problem of a steady flow of electrons between infinite plane electrodes has been treated several times before, and that the existence of the " potential hill " and the resulting formula for maximum current are well known. Very exhaustive investigations of unmodulated electron beams are to be found in the papers by Fay, Samuel and Shockley (1938), and by Plato, Keen and Rothe (1936).

The problem of velocity-modulated beams has also been treated by several writers besides Webster. A most interesting theory of beams of circular cross-section focussed by means of an " infinite " axial magnetic field has been developed by Hahn and Ramo (see Ramo, 1939). Under the assumption of a small modulation they prove the existence of " electronic waves ". In our terminology these waves could be described roughly as compressions travelling along the beam at a velocity slightly different from the mean velocity of the beam. The " electronic wave theory " is much superior to the elementary theory developed in this paper, in so far as it takes account of the finite cross-section and of the influence of the screening boundary of the drift space. It should be pointed out, though, that the boundary conditions assumed by Ramo (infinite length of beam, variation of electron velocity with distance from the axis) are not satisfied in reality, so that the results can only be regarded as qualitative. Since our discussion of velocity-modulated beams is physically more intuitive and mathematically much simpler than Ramo's, we hope that the publication of the present paper may still serve a useful purpose.

FUNDAMENTAL LAWS AND DEFINITIONS IN PHYSICS: I—OHM'S LAW *

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§ 1. INTRODUCTION

THERE are some sections of Physics the treatment of which may be varied according to taste or opinion. Much harm might be done in restricting the scope of the discussion to any particular method of treatment or to any single viewpoint. There are other matters in which it seems highly desirable to adopt greater uniformity of treatment and point of view, and a standardization of notation and terms is highly desirable.

The object of this article is to stimulate co-operative discussion with a view to discovering the best elementary treatment of Ohm's law.

§ 2. OHM'S LAW

What did Ohm discover? Is the most useful introduction to Ohm's law a study of Ohm's actual investigations? What is the most useful statement of the law?

An account of the experimental work of Ohm is given in *A History of Physics* by Florian Cajori. He first established the laws relating to the resistance of metallic wires embodied in the formula, $r = \frac{l}{a}S$. He experienced difficulties in these experiments owing to variations of his batteries, and he finally used thermo-electric elements as his sources of E.M.F. He then measured the strength of the current in a circuit, keeping the E.M.F. constant and varying the resistance of a portion of it. He found results in agreement with the formula

$$X = \frac{a}{b+x},$$

where X is proportional to the current in a conductor forming part of the circuit whose length x was varied. a and b are constants.

* Paper to be read at a joint meeting with the Science Masters' Association, 9 April 1942.

By repeating his experiments with a different E.M.F. he showed that a assumed a new value but not b. Thus he identified a with the E.M.F., and b with the resistance of that part of the circuit which he did not vary.

Thus the formula $X = \frac{a}{b+x}$ is equivalent to

$$\text{Current} = \frac{\text{E.M.F.}}{\text{Resistance}}. \qquad \ldots \ldots \quad 1$$

This work was published in 1826.

In 1827, Ohm gave a theoretical investigation of electrical conductivity in metallic conductors based on Fourier's mathematical treatment of the conduction of heat. This is referred to in the *Encyclopaedia Britannica*, Article *Ohm*, as though it constituted the whole of his work. " It may be doubted whether Ohm's investigation could have been made but for the magnificent work of Fourier on the Conduction of Heat." It will be observed that the electric conductivity of a metal was proved by Ohm to be independent of the potential difference, but the thermal conductivity was incorrectly assumed by Fourier to be independent of temperature.

The Ninth Edition of the *Encyclopaedia Britannica* contains a more appreciative account of the work of Ohm by Professor George Chrystal, but no mention is made of his experimental work. In the article on *Electricity* he says: " The theory on which Ohm based his law may be and has been disputed, but the law itself and the applications which Ohm and others have made of it are in the fullest agreement with all the known facts." Later in the same article, the equivalent of the following formula is given:—

$$\frac{E}{I} = r = \frac{ls}{a},$$

where l, a, s, relate to a metallic wire " and s is a constant depending on its material, temperature, and physical condition generally. This is Ohm's law ". 2

Ohm's actual investigations do not appear to provide a suitable introductory treatment to Ohm's law.

A committee of the British Association, consisting of Professors Clerk Maxwell, J. D. Everett, and Dr. A. Schuster, prepared a report on the testing of Ohm's law. The experimental work was carried out in the Cavendish Laboratory by G. Chrystal under the supervision of Clerk Maxwell. Quoting from the B.A. Report of 1876:

" The statement of Ohm's law is that, for a conductor in a given state, the electromotive force is proportional to the current produced ". 3

" The quotient of the numerical value of the electromotive force divided by the numerical value of the current is defined as the resistance of the conductor;

and Ohm's law asserts that the resistance, as thus defined, does not vary with the strength of the current." 4

Later, in the same report, Maxwell states:

" Ohm's law may be stated thus:—The electromotive force which must act on a homogeneous conductor in order to maintain a given steady current through it, is numerically equal to the product of the resistance of the conductor into the strength of the current through it. 5

" If, therefore, we define the resistance of a conductor as the ratio of the numerical value of the electromotive force to the numerical value of the strength of the current, Ohm's law asserts that this ratio is constant—that is, that its value does not depend on that of the electromotive force or of the current.

" The resistance, as thus defined, depends on the nature and form of the conductor, and on its physical condition as regards temperature, strain, etc., but if Ohm's law is true it does not depend on the strength of the current."

The Dictionary of Physics states the results of these experiments as showing that " the resistance of a conductor one square centimetre in section is not diminished by as much as $1/10^{12}$ of its value—assuming the temperature constant —when a current of 1 ampere traverses it. We may consider, therefore, that Ohm's law is verified and that the ratio of the E.M.F. to the current is constant ".

" Ohm's law states that in any circuit under constant physical conditions the ratio of the electromotive force to the current is a constant. This constant is known as the resistance of the circuit; its reciprocal is the conductivity."

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§ 3. STATEMENTS OF OHM'S LAW

J. J. Thomson, *Elements of Electricity and Magnetism*, 1904.

" Ohm's Law. The relation between the electromotive force and the current was enunciated by Ohm in 1827, and goes by the name of Ohm's Law.

" This law states that if E is the electromotive force between two points A and B of a wire, I the current passing along the wire between these two points, then

$$E = RI, \qquad (7)$$

where R is a quantity called the resistance of the wire ".

" The point of Ohm's law is that the quantity R defined by this equation is independent of the strength of the current flowing through the wire, and depends only upon the shape and size of the wire, the material of which it is made, and upon its temperature and state of strain.

" The most searching investigations have been made as to the truth of this law when currents pass through metals or electrolytes; these have all failed to discover any exceptions to it, though from the accuracy with which resistance can be measured (in several investigations an accuracy of one part in 100,000

has been attained) the tests to whch it has been subjected are exceptionally severe."

W. C. D. Whetham, *The Theory of Experimental Electricity*, 1923.

" Ohm's law, that the current c varies as the applied electromotive force E, may be stated in the form

$$c=kE. \qquad 8$$

where k is a constant known as the conductivity of the conductor.

" Another mode of statement, more usually adopted, is to say that

$$c=\frac{1}{R}E \quad \text{or} \quad E=Rc,$$

where R is a constant which, like the conductivity, depends only on the nature, dimensions and temperature of the conductor, and is independent of the current. This constant R is called the resistance."

R. W. Pohl, *Physical Principles of Electricity and Magnetism*, translated by Winifred M. Deans, 1930.

" In general we define the resistance r of any conductor to be the quotient

$$\frac{\text{Pressure } (E) \text{ between the ends of the conductor}}{\text{Current } (i) \text{ in the conductor}}.$$

" This quotient E/i, which is defined to be the resistance r, is not in general constant for the conductor in question. The resistance r depends in a complicated way on the current i and on numerous conditions of experiment. It is only in particular cases that the quotient E/i is constant. In these particular cases Ohm's law—which is one of our most important experimental results—is true. In words:

" The pressure E between the ends of a conductor is proportional to the current i in the conductor, or

$$E=iR. \qquad 9$$

" A metallic conductor at constant temperature is an example of a particular case where Ohm's law holds."

Alexander Russell, *Alternating Currents*, 1904 and 1914.

" Ohm proved experimentally that the ratio of the difference of potential to the current was constant so long as the conductor between the two points remained in the same physical state. This constant ratio is called the resistance of the conductor between the points, and Ohm's law may be written

$$i=\frac{e}{r}\text{"} \qquad 10$$

J. H. Jeans, *Electricity and Magnetism*, 1923.

" Ohm's Law. The difference of potential between any two points of a wire or other linear conductor in which a current is flowing, stands to the current

flowing through the conductor in a constant ratio, which is called the resistance between the two points. 11

" It is here assumed that there is no junction with other conductors between these two points, so that the current through the conductor is a definite quantity."

§ 4. E.M.F. AND POTENTIAL DIFFERENCE

In Whetham's *Electricity* the following passage occurs :—" In the science of current-electricity, it is usual to call a difference of potential an electromotive force. The name is not a happy one, for an electric force f has the physical dimensions [force/quantity of electricity], and is related to the potential-difference V, or [work/quantity of electricity], by the equation $f = -dV/dx$. The use of the name electromotive force as a synonym for difference of potential, however, is established firmly, ..."

The statements of Ohm's law numbered 1 to 8 all use the term E.M.F. synonymously with potential difference. Statements 9 to 11 employ the term potential difference and do not refer to E.M.F. The term potential difference or " pressure drop " is favoured by electrical engineers.

Some writers distinguish between the two terms, and whilst there is no advantage in retaining the use of two terms with the same meaning, there seems to be an advantage in restricting the use of E.M.F. to the voltage produced by a generator. Only a battery, dynamo, induction coil, and the like, can have an E.M.F., but anything through which a current of electricity is flowing has a potential difference equal to the strength of the current flowing multiplied by the resistance of the conductor. This potential difference is provided by the generator in the circuit. This is merely a statement of Kirchhoff's second law relating to a current-carrying network of conductors. Again, in any non-generator, the current flows from a high to a low potential, but in a generator there is a transformation of energy resulting in the electricity rising in potential in the direction of current flow. The rise in potential equivalent to the transformation of energy per unit quantity of electricity is the E.M.F. (due allowance being made for the natural fall in the potential due to resistance of the generator).

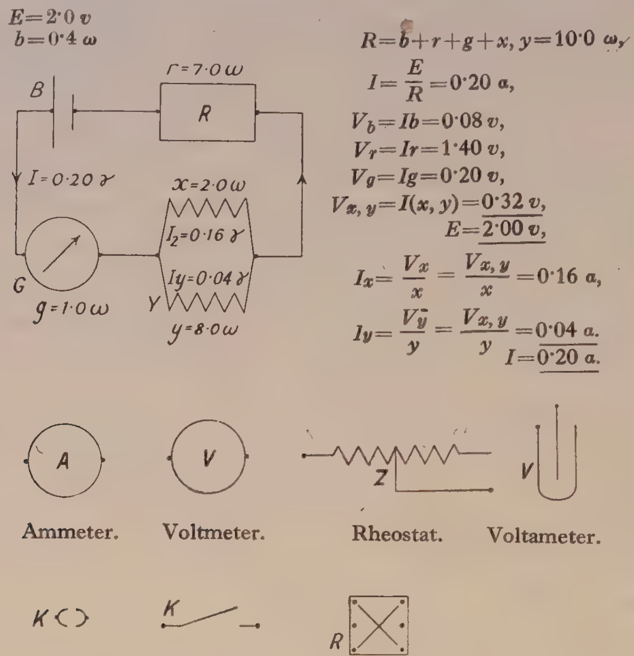
The use of the term E.M.F. with two meanings is undesirable. The E.M.F. of a cell always means the potential difference at its terminals when on open circuit and not the potential difference for the cell when it is sending a current.

§ 5. NOTATION

A standard notation is desirable in symbols, diagrams, and working. The writer uses the notation indicated in the diagram (figure 1).

A suitable Greek letter may be preferable to v for volts.

i, e, v, r, could be used for current, E.M.F., potential difference, and resistance in electromagnetic units (e.m.u.), but v and r are seldom required.



$$R=b+r+g+x,y=10{\cdot}0\ \omega,$$
$$I=\frac{E}{R}=0{\cdot}20\ a,$$
$$V_b=Ib=0{\cdot}08\ v,$$
$$V_r=Ir=1{\cdot}40\ v,$$
$$V_g=Ig=0{\cdot}20\ v,$$
$$V_{x,y}=I(x,y)=0{\cdot}32\ v,$$
$$E=2{\cdot}00\ v,$$
$$I_x=\frac{V_x}{x}=\frac{V_{x,y}}{x}=0{\cdot}16\ a,$$
$$I_y=\frac{V_y}{y}=\frac{V_{x,y}}{y}=0{\cdot}04\ a.$$
$$I=0{\cdot}20\ a.$$

Ammeter. Voltmeter. Rheostat. Voltameter.

Make and break key. Current reverser.

Figure 1.

Resistances are written external to the mesh and currents within it.

b, r, g, x, y, are the numerical values of the resistances of the circuit units B, R, G, X, Y, respectively.

§ 6. ILLUSTRATION OF OHM'S LAW

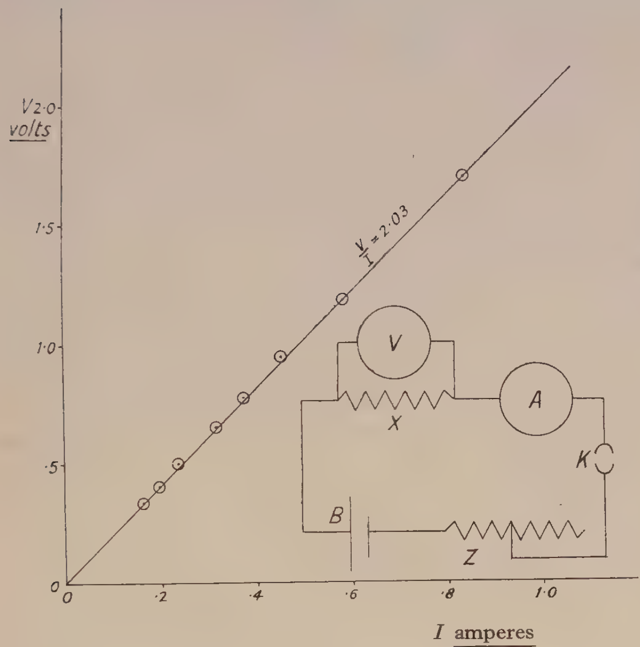
Figure 2 shows a form of apparatus suitable for beginners. The object of the experiment is to apply a series of different potential differences (V) to a metallic conductor X (a 2-ohm coil is suitable), and to observe how the current (I) through the conductor varies with the applied potential difference.

The ammeter A (range 0 to 1·0 ampere), may be calibrated with a tangent galvanometer, but this does not form part of the experiment, and a discussion of this will only distract the attention of the beginner from the main issue. Similarly, the high resistance voltmeter (V) (range 0 to 2·5 volts, resistance 500 ohms or more) may be calibrated in any way whatever, but in this experiment the scale is taken as correct.

The key K is an important part of the apparatus. The readings should be taken as quickly as is consistent with reasonable accuracy, and the current should only be allowed to flow for short periods of time to avoid the coil X being heated

by the current. It is important to direct attention to this matter, although with a manganin coil the temperature effect is well within the error of reading even if little attention is paid to this matter.

Figure 2 shows results obtained without special care and by a beginner.



V	I	$\frac{V}{I}$	$\frac{I}{V}$
1·72	0·85	2·02	0·495
1·20	0·59	2·03	0·493
0·95	0·46	2·06	0·485
0·78	0·38	2·05	0·488
0·65	0·32	2·03	0·493
0·50	0·24	2·08	0·481
0·40	0·20	2·00	0·500
0·34	0·17	2·00	0·500

Figure 2.

The low readings are accidentally more accurate than the higher readings, or it may be a compensation of scale and experimental error.

If the potential difference be regarded as the independent variable, I is plotted against V, and the graph obtained shows that I/V is constant or $I = kV$, where k is the conductance of X.

If the current be regarded as the independent variable, V is plotted against I, and the graph (figure 2) shows that V/I is constant or $V = xI$, where x is the resistance of X.

Thus the current through a metallic conductor under fixed physical conditions is directly proportional to the potential difference applied to it. This is Ohm's law.

If Ohm's law holds for every part of a circuit, then the current in a circuit whose E.M.F. is varied is directly proportional to the E.M.F. In this case $E = RI$, and this same relation holds in such a circuit if E is constant and R is varied. In a circuit whose E.M.F. is due to thermo-elements, or if the circuit contains a variable-speed dynamo with constant separately-excited field, the E.M.F. may be varied. In these circuits R is not definitely constant because

the temperature is not constant throughout the whole of the thermoelectric circuit, and the resistance of a brush contact of a dynamo varies with the current density over the contact area and with the peripheral speed of the commutator. Similarly, in a battery circuit in which the E.M.F. is constant, the relation $E=RI$ is not exactly true because the resistance of a cell varies somewhat with the current that it sends through the circuit. Thus it is not correct to say that for a complete circuit

$$\text{Current} = \frac{\text{E.M.F.}}{\text{Total resistance}}.$$

§7. UNITS

It may be advisable to distinguish between the definition and the description of a unit. The definition of a unit should be framed so that the relation of the unit to the fundamental units of length, mass, and time is clearly indicated.

Definition. An ampere is 1/10 of that current which flowing along a conductor 1 centimetre in length bent into the arc of a circle of 1 centimetre radius produces a force of 1 dyne on a unit pole at the centre of the circle.

The dimensions of current may be found from this definition. In addition the formula $f=2\pi In/10r$ immediately follows from it if the inverse-square law for the variation of magnetic force with distance be assumed. This law was verified experimentally by Maxwell.

What is called the legal or practical definition of the ampere is no definition at all, any more than the definition of a shilling is that it is equal to 12 pence. It is one description of an arbitrarily chosen chemical effect of the current. The mass of silver deposited by one ampere in one second or by one coulomb could not be ascertained until the ampere had been defined, and the value found by experiment can never be accepted as entirely accurate. In what proportions are the isotopes of silver deposited during electrolysis? Is the equivalent weight of silver determined by chemical analysis exactly the same as that found by electro-deposition?

Why is it necessary to have two units? Why not make the ampere 10 times larger?

Definition. A volt is the potential difference between two points if a joule of work is done in transferring a coulomb from one point to the other.

The dimensions of potential difference may be found from this definition. Also, assuming the law of Conservation of Energy, the formula $JH=VIt$ is immediately obtained. Again, for a conductor that obeys Ohm's law it follows that $JH=I^2Rt$ (verified experimentally by Joule).

The practical "definition" of the volt in terms of the E.M.F. of a normal Weston cadmium cell is one description of many that could be given of the volt. A volt is also the E.M.F. induced in a conductor when it cuts magnetic lines of force at the rate of 10^8 per second.

Definition. A conductor has a resistance of 1 ohm if a current of 1 ampere flows through it when a potential difference of 1 volt is applied to it.

The practical " definition " of the ohm is far from being practical. A tube of uniform cross section of 1 square millimetre 106·3 centimetres in length is impossible of realization, and what is the isotopic composition of pure mercury?

§ 8 CONCLUSIONS

1. It is desirable to standardize terms, definitions, symbols, and diagrams.

2. It is desirable to assign different meanings to the terms E.M.F. and potential difference.

3. Ohm's law applies most accurately to metallic conductors.

For a metallic conductor under fixed physical conditions, the ratio of the potential difference applied to it to the current flowing through it is constant. This is Ohm's law.

4. The resistance of a conductor is the ratio of potential difference applied to it to the current passing through it.

The resistance of a conductor is not, in general, constant.

Conductance is the reciprocal of resistance.

5. Is it necessary to have

$$\begin{aligned} 1 \text{ ampere} &= 10^{-1} \text{ e.m.u.} \\ 1 \text{ volt} &= 10^{8} \text{ e.m.u.} \\ 1 \text{ ohm} &= 10^{9} \text{ e.m.u. ?} \end{aligned}$$

Could the ampere be made equal to 1 e.m.u., and if the volt and ohm are no equated to 1 e.m.u. could each be made 10^7 e.m.u. ?

6. The practical definitions are not suitable for the theoretical development of the subject.

FUNDAMENTAL LAWS AND DEFINITIONS IN PHYSICS : II—SPECIFIC HEAT AND NEWTON'S LAW OF COOLING *

By C. W. HANSEL,
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Received 9 *December* 1941

§ 1. INTRODUCTION

THE terms thermal capacity and specific heat are not always defined in the same way. For example :—

J. Clerk Maxwell, *Theory of Heat*, 1871 and 1894.

" The capacity of a body for heat is the number of units of heat required to raise that body one degree of temperature." 1

* Paper to be read at a joint meeting with the Science Masters' Association, 9 April 1942.

" The Specific Heat of a body is the ratio of the quantity of heat required to raise that body one degree to the quantity required to raise an equal weight of water one degree." 2

Sir William Thomson, *Encyclopaedia Britannica* (9th edition). Article *Heat.*

" Definition 1. The thermal capacity of a body . . . . is the quantity of heat required to raise its temperature by one degree on the absolute thermodynamic scale." 3

" Definition 2. The specific heat of a substance is the thermal capacity of a stated quantity of it.

This stated quantity is generally understood to be the unit of mass, ..." 4

H. L. Callendar, *Encyclopaedia Britannica* (10th and subsequent editions). Article *Calorimetry.*

" The thermal capacity of a body is measured by the quantity of heat required to raise its temperature one degree, ..." 5

" The specific heat of a substance is sometimes defined as the thermal capacity of unit mass, but more often as the ratio of the thermal capacity of unit mass of the substance to that of unit mass of water at some standard temperature. The two definitions are identical, provided that the thermal capacity of unit mass of water, at a standard temperature, is taken as the unit of heat. But the specific heat of water is often stated in terms of other units." 6

Thomas Preston, *Theory of Heat*, 1904.

" The thermal capacity of a body is defined as the quantity of heat necessary to raise the temperature of the body 1° C., and the thermal capacity of a substance is the quantity of heat required to raise unit weight (one gramme) of the substance 1° C." 7

" The specific heat of a substance is its thermal capacity compared with that of water; . . . ." 8

J. H. Poynting and J. J. Thomson, *Heat*, 1906.

" The specific heat of a substance is the number of calories needed to raise 1 gramme of the substance 1° C." 9

Capacity for heat of a calorimeter is mentioned but not emphasized.

A. W. Barton, *Heat*, 1933.

" The thermal capacity of a body is the amount of heat to raise its temperature through 1° C. 10

" The specific heat of a substance is the amount of heat needed to raise the temperature of 1 gramme of it through 1° C." 11

§ 2. THERMAL CAPACITY AND SPECIFIC HEAT

In the statements 1 to 11 there are various definitions of thermal capacity, but are they of any practical importance ?

It is scarcely necessary for a beginner to know this term, and it might be omitted from examination questions.

The water equivalent of a calorimeter is not quite numerically the same as the thermal capacity of it since it is a mass of water which in an actual experiment and under actual working conditions would absorb the same amount of heat as the calorimeter absorbs. This is not quite the same as the mass of the calorimeter multiplied by its specific heat.

The term specific heat is sometimes defined as a ratio and sometimes as a quantity of heat per unit mass per unit rise of temperature. If specific heat is to enter into an equation of the form $H=ms(t_2^\circ - t_1^\circ)$, it cannot be a pure number, and for this reason I think the ratio definition should be abandoned.

Again, specific heat is the heat per unit mass (gram, pound, or kilogram) per degree (any). In this case consistent units must be used in the heat equation.

The British Thermal Unit of Heat involves the pound as the unit of mass. The heat engineer borrows this unit and uses it instead of the slug-degree-Fahrenheit unit. In Joule's determination of the mechanical equivalent of heat, the heat is in British Thermal Units and the work is in foot-pounds.

I favour the definition of specific heat given in Chambers' *Dictionary* :—

" Specific Heat, the number of heat-units necessary to raise the unit of mass of a given substance one degree in temperature."

This definition is dimensionally correct and applies to all consistent sets of units.

§ 3. NEWTON'S LAW OF COOLING

F. H. Schofield, *Dictionary of Applied Physics* (*Heat*, *Convection of*).

" Newton propounded a law to the effect that the rate of cooling of a hot body in a stream of air is proportional to the difference in temperature between the body and the air." 1

And in a footnote, " This law is often taken to apply to natural convection, though Newton expressly said that it was given for a body ' not in still air but in a uniform current of air.' "

" The heat loss by forced convection from a hot surface is proportional to the temperature difference between the surface and the ambient fluid. This has been shown by Boussinesq from hydrodynamical reasoning, by Rayleigh from the principle of similitude, and it is confirmed by a considerable mass of experimental evidence."

" Conclusions on natural convection. (1) The heat loss from a hot surface is approximately proportional to $\theta^{5/4}$, where θ° is the temperature difference between the hot surface and the ambient fluid."

See also the article on *Dynamical Similarity* in the same volume by Hyman Levy.

Engineers appear to have applied Newton's law in the form stated by Newton, that is, to a body cooling in a draught. Newton's law is a law of forced convection, and appears to hold up to high temperature if the hot body moves in a rapidly moving current of gas. The cylinder of a moving motor-cycle cools according to this law.

Textbooks of physics often state Newton's law of cooling as follows:—

"The rate of cooling of a body is proportional to its excess temperature above the temperature of its surroundings provided that this excess temperature is small." 2

According to this statement, Newton's law is not a "law" at all. All that is asserted in this statement is that a short enough part of the cooling curve at the origin is straight. This is necessarily true whatever the shape of the cooling curve.

Still worse is that form of demonstration which reconciles Stefan's fourth-power law of radiation with Newton's law of forced convection. Surely, a black body cooling in a vacuum does not cool in the same way as an ordinary body cooling in a stream of gas.

It has been stated that an ordinary thermometer heated to 20° or 30° above the temperature of the surrounding air cools according to statement 2. The following results obtained with cooling thermometers do not confirm this. The data given refer to thermometers cooling in air at temperature 20° c.

The bulb A was blackened over burning camphor. B was the same thermometer as A, but with a bright bulb (black rubbed off).

Time (min.)	0·0	0·5	1·0	1·5	2·0	2·5
A °c.	105·5	76·4	59·3	48·0	40·0	34·7
B °c.	110·0	80·0	61·2	49·2	40·8	35·5
Time (min.)	3·0	3·5	4·0	4·5	5·0	
A °c.	31·2	28·8	27·2	26·0	25·1	
B °c.	31·8	29·3	27·4	26·3	25·4	

§ 4. THE COOLING CALORIMETER

For natural convection, the heat loss from a surface is proportional to the excess temperature to the power of 5/4. Does natural convection occur when a calorimeter is used as in a determination of specific heat by the method of mixtures? What difference of temperature is necessary between a body and surroundings for natural convection to occur? Does cooling occur according to statement 2? Is there a surface film of gas on the calorimeter which is almost at the same temperature as the calorimeter and impedes both convection and radiation and transference of heat from the calorimeter to the surrounding air?

Numerical data relating to these problems would be very valuable.

Can Newton's law of cooling be illustrated with a cooling calorimeter? Statement 2, which is not Newton's law, may be approximately applicable. Since there is no forced convection, Newton's law cannot be illustrated in this way.

Examination questions

A candidate should not be asked to verify Newton's law of cooling for a cooling calorimeter.

Even in the School Certificate Examination, experiments have been set in which cooling observations provide the data from which a graph is required of rate of cooling plotted against excess temperature. It is impossible to obtain the rates of cooling with sufficient accuracy by drawing tangents to a cooling curve. In experiments of this kind, is the plot of rate of cooling against excess temperature expected to be a straight line?

The experimental data given below throw some light on the difficulties of work of this kind and the impossibility of obtaining sufficient accuracy in the measurement of temperatures taken carefully with a thermometer graduated in tenths of a degree.

100 grams of water contained in a calorimeter of water equivalent 5 grams cooled in surroundings kept at a constant temperature $t = 16 \cdot 8°$ C. The temperature of the water was $t°$ C. at time T minutes.

T min.	0	1	2	3	4	5	6	7
$t°$ C.	47·7	46·8	46·0	45·2	44·5	43·8	43·2	42·5
$t°$ C. (corrected)	47·56	46·77	46·01	45·27	44·54	43·84	43·15	42·48

T min.	8	9	10	11	12	13	14
$t°$ C.	41·8	41·1	40·5	40·0	39·4	39·0	38·4
$t°$ C. (corrected)	41·82	41·19	40·57	39·96	39·37	38·80	38·24

From the plot of log $(t - 16 \cdot 8)$ against T it is found that

$$\log (t - 16 \cdot 8) = -0 \cdot 0112T + 1 \cdot 488.$$

The corrected values of t have been calculated from this equation.

Although the uncorrected values of t are as accurate as could be observed to the nearest tenth of a degree, they are not accurate enough to enable the rate of cooling to be found with sufficient accuracy to show that a linear relation exists between the rate of cooling and $(t - 16 \cdot 8)$. Values found by drawing tangents to the cooling curve give even worse results. It may be thought that the temperatures might have been estimated to $0° \cdot 01$ C. This cannot be done owing to the sticking of the mercury in the thermometer.

According to these results the rate of cooling is proportional to the excess temperature to the power of 1·03.

§ 5. RECOMMENDATIONS

1. That the term thermal capacity be omitted from examination syllabuses and that examiners avoid the term when setting questions.

2. That specific heat be defined as a quantity of heat per unit mass per unit rise of temperature.

3. That Newton's law of cooling shall only be deemed to refer to a body cooling in a stream of gas. The name of the law should not be used to mean the proportionality of rate of cooling and excess temperature for a body cooling in any arbitrary way.

FUNDAMENTAL LAWS AND DEFINITIONS IN PHYSICS: III—MASS *

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§ 1. INTRODUCTION

THREE fundamental conceptions are those of space, time and matter. They form the background of daily experience. Space, time and matter are measured in terms of arbitrary units of length, time and mass.

It is a mistake to postpone the teaching of ideas on mass on the score of difficulty (see Quotation 94 and the B.A. *Report*). The difficulty is not one of *ideas* but of *words*. A child becomes familiar with the popular use of the word "weight" in the sense of "mass" and uses the word *weight* for *mass*. What does a child know about gravitational attraction? To what extent is a child interested in the gravitational pull on a bag of sweets? The child is very definitely interested in the mass of the material, the quantity of delectable substance, not in the gravitational pull on it. The beginner in science must be trained to use his words carefully and correctly. The same word should not be capable of two different scientific interpretations. No slovenliness of record or speech should be tolerated. A similar difficulty occurs in the use of the words *heat* and *temperature*. A beginner uses the word *heat* in the popular sense of *temperature*. Persistent correction ultimately eradicates the habit.

The distinction between mass and weight should be taught early in the science course. The contention that the idea of weight is familiar and the idea of mass obscure is an incorrect way of stating that popularly the word *weight* is used in many different senses (see later) and usually in the same sense as the scientific term *mass*. Actually, this initial misuse of a word may be turned to instructional advantage if the distinction between mass and weight is emphasized and the correct use of these terms is insisted upon. A beginner has no difficulty in grasping Newton's idea of gravitational force between objects. The weight of a body is defined to be the gravitational force of the earth on it, and varies from place to

* Paper to be read at a joint meeting with the Science Masters' Association, 9 April 1942.

place. A beginner may be led to predict that a body will have no weight at the centre of the earth. It is easily realized that a spring balance directly measures weight.

It is important in a science course to use a beam balance early. It is not necessary to know the theory of a beam balance before using it to measure mass, any more than it is necessary to know the construction and theory of a clock before using it to measure time. The young beginner is quite capable of understanding that the result of weighing on a beam balance is the same everywhere, and hence it does not measure weight, which is not the same everywhere. A young child has usually played with a toy set of scales and " weights ", and in any case he knows that a pair of scales is not used to measure gravitational attraction, but to measure the quantity of all kinds of different materials. Quantity of matter is mass—the beam balance measures mass.

From now on, all the results of weighing with a beam balance are recorded and spoken of as mass, and results of weighing with a spring balance as weight. If in all lessons the correct terms are used, the habitual careful and correct use of the terms mass and weight becomes established. The science teacher has laid the foundation for future extension of the work.

A few teachers of mechanics ignore the term *mass* completely. It will be generally agreed that this is undesirable. What is even more undesirable is for the teacher of mechanics to mention the *term* but not to use the *idea*. It is unfortunate that teachers of science, mathematics and engineering do not always collaborate to the fullest extent to give to each other the greatest possible assistance in developing and using this important and fundamental idea of mass. It is most desirable that a uniform treatment and a uniform system of symbols and formulae should be used by all who are concerned in the teaching of mass and related subject-matter. It is essential that all those engaged in scientific instruction should abide by the correct use and meaning of scientific terms and train the pupil accordingly. By the time the student begins the study of dynamics he should have established the habit of using the terms mass and weight carefully and correctly. The habitual correct use of the term mass may now be enlarged and used in connection with force, momentum, kinetic energy and, at a later stage, moment of inertia.

§ 2. SYSTEMS OF UNITS

Some teachers of mathematics and engineering use only one system of units. Some use three systems and a fourth hybrid " system ", more accurately described as chaos than system (see *A First Dynamics*, by C. S. Jackson and W. M. Roberts, 1909, p. 311). It will be generally agreed that a student should not only be able to use any system of units, but he should use them freely and with equal facility. In the interests of sound training, and to avoid confusion and vagueness, any so-called *fourth system using a mixture of units from more*

than one system, or using two units for the same quantity, or one name for two units (for example, gram—gram weight—dyne), *should be rigorously excluded.*

No one should claim that all discussion should be in terms of gravitational units, and those with a predilection for gravitational units have no right to relegate the poundal to a subsidiary position as an auxiliary unit only (see later, Clause 20, B.A. *Report*, 1907). The poundal is not an educational convenience —it is a unit of force philosophically conceived and quite independent of the gravitational attraction of the earth. It does not vary from place to place, but is a fixed unit which may be used quite naturally and universally in all dynamical problems.

The system of units used by engineers is not happily conceived. A horse-power is 33,000 foot-pounds per minute. This necessarily involves the pound weight as the unit of force, and the name for the corresponding unit of mass has not come into common use. Occasionally, the engineer borrows a unit of mass from the F.P.S. system as a matter of popular convenience, and this leads to chaotic units. If the density of water is quoted as 62·5 pounds per cubic foot on the back of a slide rule, this is pounds mass : otherwise it would be dimensionally incorrect. The pound mass properly belongs to the F.P.S. system. Again, the British thermal unit involves the pound mass. In working out the mechanical equivalent of heat from Joule's original experiment, the heat is reckoned in British thermal units involving pounds mass, and the work is in foot-pounds involving pounds weight. If the engineer must use these units (and it is not expected that he will change them) he should recognize that he has borrowed the pound mass for the time being from the F.P.S. system, but on no account should such units used together be regarded as an alternative " system " of units.

In the Preface to *Hydrostatics and Elementary Hydrokinetics*. by George M. Minchin, the following passage occurs:—" Some long controversies have recently taken place between the representatives of the practical engineers, on the one hand, and those who may be termed the scientific reformers, on the other, with regard to the way in which we should speak of the common gravitation measure of force. The former speak of ' a force of one pound ', for example, while the latter prefer ' a force of one pound weight '. As in the last edition of my Statics, I have everywhere throughout the present work adopted the latter mode of speaking, because I regard the distinction between *mass* and *weight* of a body as absolutely fundamental—the former being an essential and invariable, while the latter is a purely contingent and variable, attribute of the body. Indeed, were it not for the fact that the Earth is nearly spherically symmetrical as regards shape and density about its centre, it seems scarcely possible that men, communicating with each other over long distances, could ever have adopted the former mode of speaking " 1

Those who receive elementary instruction in mechanics will ultimately be trained as mathematicians, astronomers, physicists, engineers of all kinds, civil,

mechanical, electrical, aeronautical, marine, etc. All these students receive their early training together, and their early studies should lead quite naturally and without difficulty to any more advanced form of training. Those teachers whose training has been specialized and who attach great weight to gravitational units should co-operate to the full in an attempt to ease the path of the beginner without interfering with the rigour of his training or narrowing his outlook on this important question of units.

The three systems of units which should be taught to all students from the very beginning are the C.G.S., the F.P.S. and the gravitational systems. A student should work problems in all these units. There will be some difference of opinion with regard to the inclusion of the F.P.S. system. This difference of opinion is a sure and certain indication that units are not being taught in the right way. If the treatment of units is satisfactory, the inclusion of F.P.S. units will help rather than hinder the student in getting a clear grasp of his problems. A little extra time must be devoted to the matter, but this is time well spent. Any student who can use the C.G.S. system has no difficulty whatever with the F.P.S. system, and conversely. Still further, if the gravitational unit of mass is given a name in the same way as the other units, any student who knows how to use one system has no difficulty with any other system. This is why uniformity of treatment is so essential. The treatment of any problem and the symbols and formulae used should be the same whichever set of units is used. It is most undesirable to develop the subject by any method which requires modification in passing from one system of units to another or which requires modification in any way at a more advanced stage of study. Moreover, a student should use all systems of units with equal facility and in the same way.

§ 3. UNITS OF MASS

The standard pound and the standard kilogram are the legal units of mass (see Quotation 59). A gram is one thousandth of a kilogram. 1 cubic centimetre of water at 4° c. weighs approximately 1 gram, in fact 1·0000 cubic centimetre of water at 4° c., and at a pressure of 1 standard atmosphere weighs 1·0000 gram. Reference to the temperature of maximum density of water should be avoided. For greater accuracy of expression, the isotopic composition of water should be specified.

A litre used to be regarded as a cubic decimetre or one thousand cubic centimetres. Why change this definition? The graduation marks on a burette or a pipette are not visibly altered from graduation marks in cubic centimetres, and, in any case for accurate work, these instruments will be calibrated by weighing. It would seem desirable to return to the original definition of the litre and for measuring vessels to be graduated in c.c. instead of ml. As things now are, the instrument maker appears to be the only one concerned with the millilitre. Measuring vessels are graduated in millilitres, and the user calls them cubic centimetres.

§ 4. THE CONCEPT OF MASS

The notion of mass, apart from its exact quantitative definition, is not a difficult one. The inertia of objects and the quantity of matter in food, drink and raw materials are well known to everyone and matters of everyday experience. Rough estimates of mass are commonplace.

E. Mach, in *The Science of Mechanics*, states:—"With regard to the concept of ' mass ', it is to be observed that the formulation of Newton, which defines mass to be the quantity of matter in a body as measured by the product of its volume and density, is unfortunate. As we can only define density as the mass of unit volume, the circle is manifest. Newton felt distinctly that in every body there was inherent a property whereby the amount of its motion was determined, and perceived that this must be different from weight. He called it, as we still do, mass; but he did not succeed in correctly stating this perception." 2

Consider a heavy object (a 56-lb. weight or large bucket of shot or sand) suspended by a long and strong cord from a rigid and high support. The weight of the object is balanced by the tension in the cord. Horizontal displacement or rotation of this massive object is resisted. Motion of translation or rotation cannot readily be reduced or increased. The law of inertia relates to this universal property of matter of resisting change in its state of rest or of uniform motion. This universal property of matter and a constant property for any particular object is called inertia and is measured as mass.

Newton's first law of motion provides a definition of mass and a definition of force.

§ 5. DEFINITION OF MASS

Science of Mechanics, by E. Mach, 1907.

" In the first place we do not find the expression ' quantity of matter ' adapted to explain and elucidate the concept of mass, since that expression itself is not possessed of the requisite clearness." 3

" If, however, mechanical experiences clearly and indubitably point to the existence in bodies of a special and distinct property determinative of *accelerations*, nothing stands in the way of our arbitrarily establishing the following definition:—

" All those bodies are bodies of equal mass, which, mutually acting on each other, produce in each other equal and opposite accelerations." 4

This definition of equal masses is capable of illustration with a ballistic pendulum.

" No *logical* necessity exists whatsoever, that two masses that are equal to a third mass should also be equal to each other." *Experience* shows that they are. 5

" All uneasiness will vanish when once we have made clear to ourselves that

in the concept of mass no theory of any kind whatever is contained, but simply a fact of experience." 6

Newton's second law of motion leads to the relation

$$P = kma,$$

where k is a constant which is equal to unity if unit force produces unit acceleration in unit mass. In this case $P = ma$, a relation which is true for any system of units. This relation defines the unit of force if the unit of mass has already been defined as in the C.G.S. and F.P.S. systems. In the gravitational system, the unit of force is defined first and the relation $P = ma$ then defines the unit of mass. All the fundamental units and most of the derived units have a name in all systems of units. Surely some name should be given to the unit of mass in the gravitational system. The name *slug* has been suggested and might well be adopted.

§ 6. THE GRAVITATIONAL UNIT OF MASS

It seems urgently desirable for those who use the gravitational system of units to use a set of consistent units. If the pound weight is the engineers' unit of force, the pound mass should *not* be regarded as the engineers unit of mass. The user of gravitational units sometimes borrows the pound mass from the F.P.S. system as a matter of popular convenience and not as a matter of philosophical virtue or scientific rigour. The use of the pound as a unit of mass as well as a unit of force cannot fail to cause confusion and difficulty, particularly in the case of beginners. Such chaotic use of units should be recognized as chaotic and unscientific, and as such should be avoided. No conjoint use of chaotic units should be called a system of units.

Quotations 44 to 51 are from the writings of well-known engineers and professors of engineering. These quotations indicate that the engineer is fully alive to the importance of mass in engineering discussions and in dynamical equations. Professor Perry says " we have no name for unit of mass ", and attempts to justify this by adding " the engineer never has to speak of the inertia of a body by itself ". (Quotation 46 indicates that the term " inertia " is here used as synonymous with " mass ".) This last remark is quite inconsistent with Perry's treatment of the inertia of moving parts of an engine. In Perry's *Steam Engine* (1899), Chapter XXIX, many references to mass occur; for example:—

" If it were possible to imagine the effect of the mass of the connecting rod to be the same as that of two masses at its ends, it would be easy to balance engines." 7

" A mass m whose centre of gravity is at a distance r from the axis: it is between the wheels at the lateral distances l_1 and l_2. What masses of the wheels will balance it? " 8

"There will be a mass of $\frac{276}{32{\cdot}2} \times \frac{7}{15}$ or 4·00 moving with acceleration $\ddot{s}$ and a mass $\frac{276}{32{\cdot}2} \times \frac{8}{15}$ or 4·56 on the crank-pin." 9

In general, users of gravitational units do not give the gravitational unit of mass a name. In the preface to Professor Worthington's *Dynamics of Rotation* (1902), he says:—

"In the interests of clear teaching . . . . I have ventured to give the name of a slug to the British engineers' Unit of Mass." 10

Mr. Awbery tells me that the slug is used in the hundreds of reports and memoranda of the Aeronautical Research Committee and many other aeronautical publications. Why has not this name been adopted universally? Professor Worthington was one of the Committee responsible for the B.A. *Report*, 1907. This Report makes no reference to a matter of fundamental urgency and importance—a name for the gravitational unit of mass. Quotations 14 and 15 indicate the difficulty which the engineer has in expressing a quantity such as moment of inertia in the absence of any name for the unit of mass which is involved.

§ 7. MOMENT OF INERTIA

Quotations 11 and 12 indicate the unfortunate consequences of using pound weight and pound mass (the same name for units of different dimensions). All formulae must be duplicated and still there is confusion, due to chaotic units.

W. J. M. Rankine, *Applied Mechanics*, 1882.

"The moment of inertia of an indefinitely small body, or 'physical point' relatively to a given axis, is the product of the mass of the body, or of some quantity proportional to the mass, such as the weight, into the square of its perpendicular distance from the axis: thus, in the following equation,

$$I/g = mr^2 = Wr^2/g.$$

r is the perpendicular distance of the mass m, whose weight is W, from a given axis; and the moment of inertia, according to the unit employed, is either I or I/g; the former when the unit is the moment of inertia of an unit of *weight* at the end of an arm whose length is unity, and the latter when the unit is the moment of inertia of an unit of *mass* at the end of the same arm. For the purposes of applied mechanics the former is the more convenient unit, ..." . . 11

"In British measures, moment of inertia will be expressed by the product of a certain number of pounds avoirdupois into the square of a certain number of feet.

$$I = \Sigma \,.\, Wr^2, \quad E = a^2 I/2g,$$

where a = angular velocity, E = energy of rotation about an axis through the centre of gravity." 12

Louis Toft and A. T. J. Kersey, *Theory of Machines*, 1939.

"$I = \Sigma . mr^2$, $C = Ia$" 13

"I is known as *moment of inertia*, and if m is in engineers' units, and r in feet, I is measured in mass (feet)^2 units." 14

In the worked examples, moments of inertia are given in engineers' units, the mass being reckoned in engineers' units.

W. E. Dalby, *Steam Power*, 1915.

An answer to a numerical example on a fly-wheel is stated as "6·34 W/g. ft^2 units." 15

Quotations 14 and 15 indicate that a name for the engineers' unit of mass is required. A mass could then be stated in slugs and a moment of inertia in slug-feet^2

The following formulae apply to any consistent system of units:—

Any theorem in linear motion may be applied to angular motion by making the following substitutions:—

Moment of Inertia I for Mass m.

Torque or Couple G for Force F.

Angular displacement and its derivatives θ, ω, α for linear displacement and its derivatives s, v, a.

Thus $G = I\alpha$ is parallel with $F = ma$,

K.E. $= \frac{1}{2}I\omega^2$,, ,, K.E. $= \frac{1}{2}mv^2$,

Work $= G\theta$,, ,, Work $= Fs$.

If chaotic units and formulae are rejected, one formula is applicable in any system of units.

§ 8. DENSITY

Quotation 59 is a model of verbal and legal accuracy. Unfortunately, what is here called the Weight is only the Weight where nobody ever weighs. The operation of weighing described in Quotation 59 always yields the same result for the same body, no matter where it is carried out. The result of the operation of weighing on a beam balance is by general scientific consent called the mass of the body. The word mass should be on the tip of the tongue of the beginner all the time, his note-book entries should record mass, and the word weight should only be used when the gravitational attraction of the earth is involved.

The only definition of density which is numerically and dimensionally true is mass of unit volume, [D] = [ML^{-3}]. I do not favour any alternative. A density in pounds per cubic foot is in F.P.S. units; density is not pounds weight per cubic foot. If an engineer expresses a density in pounds per cubic foot, he has borrowed the F.P.S. system as a matter of popular convenience, but pounds weight per cubic foot is not correct.

The engineer uses the pound weight as the unit of force, but on occasion he uses the pound as the unit of mass. As remarked above, if the density of

water is stated to be 62·5 pounds per cubic foot on the back of a slide rule it is not certain whether the pound weight is indicated or the pound mass, nor, for popular practical purposes, is it necessary to know. In this case, slugs per cubic foot would not be so practically useful, but it is only the expression of density which is numerically and dimensionally true for use in dynamical formulae and equations, unless the F.P.S. system is used.

For example, quoting from Kermode's *Mechanics of Flight*:—

"Our formula becomes $R = K\rho AV^2$, R being in lb. force, ρ in slugs per cub. ft. (approx. 0·0024 slugs per cubic foot under standard conditions), A (sq. ft.), V (ft./sec.) and K (no units)." 16

Whatever apparent confusion of units and dimensions there may be in engineering calculations, this is not due to lack of care or accuracy on the part of engineers. Writers on engineering subjects usually state their results with great care and accuracy; for example, statements 14 and 15 look clumsy, but there are no better ways of expressing the result in the absence of a name for the engineers' unit of mass.

§ 9. SPECIFIC GRAVITY

Specific gravity is relative density—the ratio of the density of a substance to the density of a standard substance (usually water at 4° C. or air or hydrogen at S.T.P.). When the chemist says that the density of a gas is half its molecular weight, he means specific gravity and not density. Evidently specific gravity is equal to the mass of the substance divided by the mass of an equal volume of standard substance or the volume of a certain mass of the standard substance divided by the volume of an equal mass of the substance.

How is the Principle of Archimedes to be stated? The principle deals with forces, not masses. It is the apparent loss of weight which is equal to the weight of liquid or gas displaced. But measurements made with a beam balance are not the weights, but the masses which are proportional to these weights. The use of the term gram weight should be avoided.

Example: Using a beam balance, a solid weighs 100 grams in air, 80 grams in water, and 75 grams in a liquid. Find the density of the solid and of the liquid.

Mass of solid in air, 100 grams.
Mass of solid in water, 80 grams.
Mass of solid's volume of water, 20 grams.
Mass of solid's volume of liquid, 25 grams.
Specific gravity of solid = 100 grams/20 grams = 5.
Specific gravity of liquid = 25 grams/20 grams = 1·25.
Density of solid = 5 grams per c.c. or $5 \times 62{\cdot}5$ lbs./c.ft.
Density of liquid = 1·25 grams per c.c. or $1{\cdot}25 \times 62{\cdot}5$ lbs./c.ft.

In this example, if the measurements are in pounds the wording and calculation remain the same if pound is substituted for gram.

If a spring balance is used, weight is substituted for mass.

§ 10. PRESSURE

Pressure (P) is force (F) per unit area (A).

Pressure may be stated in dynes per square centimetre, or in poundals per square foot or in pounds weight per square foot.

Pressure in grams weight per square centimetre should be avoided. Head of water is more satisfactory. Pressure in pounds per square foot obviously means pounds weight per square foot, and is shorter for marking on a pressure gauge.

§ 11. THE CHAOTIC GRAM WEIGHT

It will be generally conceded that much confusion has arisen with regard to units and formulae owing to the use of the pound weight and the pound mass in the same problem or formula. This chaotic use of units is now so firmly established in connection with certain types of problem that the difficulty cannot now be entirely eradicated. It is, however, desirable for the student to recognize chaotic units whenever they appear, and to convert them into rational units at once. It is important to change the chaotic unit first and to manipulate the rationalized unit later. Special chaotic formulae for chaotic units naturally lead to utter chaos.

It seems obvious that the introduction of chaotic units is undesirable, and particularly is this the case when the gram weight is used as a unit of force. The dyne is the C.G.S. unit of force and the gram is the C.G.S. unit of mass. The gram weight is a chaotic unit, unwanted and entirely unnecessary. Past experience, quite apart from other important considerations, should have prevented the introduction of such a dangerous source of confusion. The gram weight should be rigorously rejected and carefully avoided.

Similarly, a pressure in grams weight per square centimetre is not rationally expressed. In C.G.S. units a pressure is in dynes per square centimetre. To express forces in grams weight or pressures in grams weight per square centimetre is to acknowledge defeat in the application of a rational system of units (C.G.S. or F.P.S.). Already there are at least four different styles of working in pound units. Are there to be four more in gram units? Will the dyne mass be invoked at a later stage? The dyne mass is no more and no less chaotic than the gram weight.

The use of the gram weight as a unit of force is far worse than the use of the pound mass occasionally by those who more often use gravitational units. The gram weight is either entirely unnecessary and chaotic or it is used in preference to the dyne. The teacher of mechanics who uses the formula $P/W = a/g$ seeks refuge in the gram weight on those very rare occasions when problems are considered which should be worked in C.G.S. units. He is aware that utter confusion prevails when this formula is used to obtain a result in dynes, partly because such results are seldom contemplated. The introduction of the gram weight as a unit of force is a confession of failure to use C.G.S. units. Most of

those educated in this way will be quite incapable of studying quantitative magnetism and electricity until the whole of the past pernicious training has been eradicated. It will then be necessary to start afresh with units and finally to deal with unit pole, etc. Even so, the position of the student is always precarious. There may be regression at any time and consequent confusion of ideas.

To reaffirm a matter of the highest importance. There is no reason whatever to use the gram weight as a unit of force or to express pressures in grams weight per square centimetre. There is every reason to discountenance these spurious units. They are used solely by those who employ methods and formulae only applicable to gravitational units and who seldom work a problem in C.G.S. units except under compulsion. Such half-developed teaching should be penalized. Every student of mechanics should be able to work problems in C.G.S. units—forces should be in dynes and pressures in dynes per square centimetre. No other units of force or pressure are necessary or should be tolerated. It should not be necessary for a teacher of magnetism and electricity to explain the meaning of the term *dyne* to those who have been studying dynamics. The matter becomes serious if the science teacher finds it impossible for the past training of the student in dynamics to adapt itself to scientific uses.

Lancelot Hogben, *Science for the Citizen*, 1937.

" Force = Mass × Acceleration.

" 1 poundal = force required to impart accel. of 1 ft. per sec^2 to mass of 1 lb.

" 1 dyne = force required to impart accel. of 1 cm. per sec^2 to mass of 1 gm." 17

" In the international system, the unit of force is one *dyne*." 18

§ 12. NEWTON'S LAWS OF MOTION

Professor Thomas Case, in *Lectures on the Method of Science*, refers to Newton's laws as follows:—

" Newton begins with the three axioms or laws of motion: the law of inertia of rest and motion, the law of proportion of force to effect, and the law of reciprocity, or equal action and reaction. He gives inductive evidence of these three laws. Though we know of no body altogether unimpeded, yet there are in our experience many instances of bodies moving straight forward the more freely the less they are impeded, which enable us to deduce the law of inertia in the form that every body perseveres in its state of rest or motion *so far* as it is not affected by an impressed force. Secondly, the proportion of force to effect is obvious to experience. The third law had been already deduced for the force of impact by Sir Christopher Wren's experiments before the Royal Society, which proved that, if one body impinge upon another, and by its force change the motion of the other, that first body also, because of the equality of the mutual pressure, will undergo an equal change in its own motion towards the contrary

part. Newton repeats Wren's experiments, and, having added fresh instances from magnetism and other attractions, extended the law from impact to all forces, and induced it in the universal form, action and reaction are always equal and opposite. Thus did Newton finally establish the main principles of dynamical mechanics by the empirical method of Bacon. But he went beyond induction, which never could have discovered the imperceptible link between the sun and its planets. The sequel is deduction; and even under the head of *Axioms* he immediately uses the third law, as induced from impact, to deduce that the whole momentum of bodies acting reciprocally on one another, together with the state of their centre of gravity, is conserved. So true is it that induction leading to deduction is the most fruitful method of natural science." . . . 19

Professor F. Gotch, in *Lectures on the Method of Science*, says:—

" Newton's enunciation of the law of gravitation is so familiar that I might merely mention it, but I desire to do more than this, because I do not find myself in complete accord with the view already put before you by my colleague, Professor Case, who has selected this as a typical example of logical deduction. For my own part I regard Newton's statement that every particle of matter in the universe exercises an attractive force on every other particle of matter, which varies inversely as the square of the distance between them, as an induction, the conception of a creative mind gifted with imagination. Professor Case himself stated that Newton passed from terrestrial to celestial mechanics. In the language of Tyndall, this ' passage from a falling apple to a falling moon ' was a stupendous leap of the imagination, for his enunciated law applies in conception to the universe, thus extending into boundless space and persisting through endless time." 20

Space does not permit any further general discussion of Newton's laws, but admirable expositions are to be found in Cox's *Mechanics* and Mach's *Science of Mechanics*. Quoting from Mach:—

" Newton's sense of *what* fundamental concepts and principles were required in mechanics was admirable. The *form* of his enunciations, however, as we shall later indicate in detail, leaves much to be desired. But we have no right to underrate on this account the magnitude of his achievements; for the difficulties he had to conquer were of a formidable kind, and he shunned them less than any other investigator." 21

§ 13. THE FUNDAMENTAL DYNAMICAL RELATION

Newton's second law indicates that the ratio of the force (F) acting on a body to the acceleration which that force gives to the body (a) is constant.

The ratio F/a is the mass of the body.

Hence $F=ma$, where m is the mass of the body.

Thus mass is the coefficient of acceleration in the fundamental dynamical relation $F=ma$ (see Quotation 74).

Since the weight (W) of a body acting on its mass m produces acceleration g of the body, $m = W/g$.

In engineers' units:

$$\text{Mass in slugs} = \frac{\text{weight in pounds weight}}{32\cdot 2 \text{ ft./sec}^2}.$$

For example, 64·4 pounds mass = 2·00 slugs.

The relation $F = ma$ indicates that unit force produces unit acceleration in unit mass. Thus,

1 dyne produces 1 cm./sec^2 acceleration in mass 1 gram.
1 poundal produces 1 ft./sec^2 acceleration in mass 1 pound.
1 pound weight produces 1 ft./sec^2 acceleration in mass 1 slug.

The first statement defines the dyne, the second defines the poundal, and the third defines the slug.

Thus, if the corresponding units of work be added, the following table of units is obtained:—

System	Length	Mass	Time	Force	Work
C.G.S.	CENTIMETRE	GRAM	SECOND	Dyne	Erg
F.P.S.	FOOT	POUND	SECOND	Poundal	Foot-poundal
Gravitational	FOOT	Slug	SECOND	POUND WEIGHT	Foot-pound

Fundamental units are printed in small capitals.

The formula $F = ma$ is in the simplest possible form for direct use and concrete comprehension. Any attempt to modify this relation in order to introduce weight (W) into it instead of mass naturally leads to confusion of units and unnatural thinking. $F = ma$ is the only relation which is appropriate to problems dealing with force and acceleration.

A student may be called upon to work a problem stated in chaotic units. This is no excuse for introducing chaos into the fundamental relation. The procedure should be to remove the chaotic units and then apply the formula $F = ma$. For example : What uniform force produces an acceleration of 10 ft./sec^2 when acting on a body of mass 64·4 pounds ?

This question is set in F.P.S. units. The natural solution is

$$m = 64\cdot 4 \text{ pounds}, \; a = 10 \text{ ft./sec}^2, \; F = ma = 644 \text{ poundals}.$$

If the answer is expected in pounds weight the question should be worded: What force in pounds weight produces an acceleration of 10 ft./sec^2 when acting on a body of mass 64·4 pounds ($g = 32\cdot 2$ ft./sec^2).

The properly trained student will immediately sense chaotic units. The result is required in gravitational units. Hence all quantities must be expressed in gravitational units before substitution in the formula $F = ma$:

$$m = 64\cdot 4/32\cdot 2 \text{ slugs} = 2\cdot 00 \text{ slugs}, \; a = 10 \text{ ft./sec}^2$$
$$F = ma = 2\cdot 00 \times 10 \text{ pounds weight} = 20 \text{ pounds weight}.$$

If, in the previous question, the weight of the body is 64·4 pounds weight, the units are not chaotic. Since $W=mg$,

$$m=64{\cdot}4/32{\cdot}2=2{\cdot}00 \text{ slugs.}$$
$$F=ma=2{\cdot}00\times 10=20 \text{ pounds weight.}$$

It is emphasized that chaotic units should be corrected before substituting in the formula $F=ma$. It is *not* advocated that chaotic units should induce a student to use a chaotic formula.

§ 14. THE B.A. REPORT OF 1907

"*The Teaching of Elementary Mechanics.*—Report of the Committee, consisting of Professor Horace Lamb (Chairman), Professor J. Perry (Secretary), Mr. C. Vernon Boys, Professors Chrystal, Ewing, G. A. Gibson, and Greenhill, Principal Griffiths, Professor Henrici, Dr. E. W. Hobson, Mr. C. S. Jackson, Sir Oliver Lodge, Professors Love, Minchin, Schuster, and A. M. Worthington, and Mr. A. W. Siddons, appointed for the Consideration of the Teaching of Elementary Mechanics, and the Improvement which might be effected in such Teaching.

"Clause 11. The impression that the weight of a body is in reality a single force acting at its centre of gravity should be guarded against.

"Clause 19. It is convenient to treat elementary problems on the accelerations produced by forces by simple proportion,

$$\frac{\text{force acting}}{\text{weight}}=\frac{\text{acceleration produced}}{g},$$

using the fact that a body's own weight produces acceleration g; and it is convenient to postpone the consideration of mass or inertia until such problems have been discussed.

"Clause 20. Students ought to know the meaning of 'absolute measure'; that is, they should be able to interpret all fundamental equations such as $E=\frac{1}{2}mv^2$, or $F=mdv/dt$, in any consistent system of units whatever. They should learn that for certain purposes the C.G.S. system of units is convenient; and for certain other purposes the units employed by British engineers may be convenient; they should not be dominated by any system, but able to use them all. The *poundal* and other such educational conveniences should be used as auxiliary units only, final results being expressed in units to which practical people are accustomed, so as to be generally intelligible."

Clauses 19 and 20 are extraordinary in that so many members of the Committee responsible for this Report have never indicated agreement with these clauses in their writings (see Appendix 1).

Clause 19 advocates the use of the relation $F/W=a/g$. Clause 19 also suggests that the idea of mass should be shelved until problems on $F/W=a/g$ have been discussed.

I venture to think that Clause 19 has done more mischief in the teaching of mechanics than the aggregate of all other ill-conceived suggestions that have ever been made in the teaching of this subject. The effect of these suggestions has led to the appearance of school textbooks of mechanics which do not mention the term mass at all, and which ignore C.G.S. units entirely. In others, the total space devoted to mass and C.G.S. units is about three consecutive pages, including the examples. What is so obvious in every case in which the formula used is $F/W = a/g$ is that it is quite unsuitable for dealing with any system of units except the gravitational system. Again, there are textbooks of science in which, after a whole chapter has been devoted to inertia and the formula $F = ma$ has been developed quite happily, there comes a sudden urge to pander to the recommendations of this *Report*, and an unnatural twist is given to the treatment in order to mention the formula $F/W = a/g$. Some teachers of mechanics use two sets of formulae throughout, one for C.G.S. units and the other for gravitational units. The cast-iron recipe is: if the body weighs m grams use $F = ma$, and if the body weighs W pounds weight use $F/W = a/g$.

Clause 20 recommends that a student should be able to interpret $E = \frac{1}{2}mv^2$ and $F = mdv/dt$ in any consistent system of units whatever. $F/W = a/g$ is the worst possible preparation for this purpose, and leads to confusion when C.G.S. or F.P.S. units are used. How is this formula to be applied to C.G.S. or F.P.S. units?

Consider the simple problem: "What force acting on a body weighing 10 grams produces an acceleration of the body of 20 cm./sec^2?

$W = mg = 10 \times 981$ dynes (using the formula $F = ma$—or is there any alternative to the use of this formula?).

$$a = 20 \text{ cm./sec}^2, \quad \frac{F}{10 \times 981} = \frac{20}{981},$$

giving $F = 200$ dynes.

Can anything be more fatuous?

Alternatively, $W = 10$ grams weight, $a = 20$ cm./sec^2, $F/10 = 20/981$, giving $F = 200/981$ grams weight.

This way of working introduces an unwanted unit of weight into the C.G.S. system, which seems worse than the previous method.

The formula suggested in Clause 19 is not consistent with the recommendation to use any system of units contained in Clause 20.

Clause 20 continues: "The poundal and other such educational conveniences . . . . generally intelligible."

This statement has already been considered in part. The poundal has been used by many mathematicians and physicists for philosophical rigour and not for educational convenience. Moreover, the statement under examination is inconsistent with the recommendation made earlier that "students . . . . should be able to interpret all fundamental equations . . . . in any consistent system of units whatever."

A student properly trained in the use of all units will express his result in the unit most appropriate to the problem or, maybe, in more than one way. The practical people mentioned in the *Report* are not defined, and their predilection for any particular system of units is somewhat indefinite, but presumably the same as the writers of the *Report*.

In view of Clause 11, what mental picture is a student supposed to conjure up when working a simple problem on force involving the application of $F/W = a/g$? Or, is the formula to be applied without forming any mental picture of the problem? What diagram can a student draw to help him to crystallize his ideas and data?

The formula $F = ma$ involves no ratios at all. A spring balance or a cord passing over a pulley to a weight F pulls the body horizontally along a smooth surface, and its displacements after equal intervals of time show ever increasing amounts. A student can visualize motion according to $F = ma$, but how can he picture the process $F/W = a/g$? How can a student visualize two ratios simultaneously?

It would be very interesting to know more details of the discussion which preceded the publication of this *Report*, and particularly that relating to Clauses 19 and 20. What were the views of various members of the Committee and to what extent was opinion divided with regard to Clauses 19 and 20? Various quotations in Appendix 1 indicate the views of some of the members of the Committee as expressed in their writings.

I have been somewhat critical of Clauses 19 and 20 of the *Report*, which relate to the teaching of " Mass ". I should, however, support most of the other recommendations.

§ 15. THE FORMULA $F/W = a/g$

This formula is undesirable for the following reasons:—

1. It is not suitable for use with C.G.S. or F.P.S. units.

2. The formula introduces the weight of the body into problems in which the weight of the body is not directly involved.

3. The formula avoids all reference to mass.

4. The formula separates quantities which should naturally come together. $F = Wa/g$ is better, but this is $F = ma$.

5. This formula and all other formulae in the dynamics of translation and the dynamics of rotation must be duplicated unless the student is confined to gravitational units.

6. Those who use this formula commonly presuppose a late development, or the complete elimination of, the idea of mass.

7. It is impossible to visualize the simplest force problem in terms of this formula.

8. The formula lacks experimental and historical background.

The only formula readily applicable to all systems of units is the simplest possible formula, $F=ma$.

It is of interest to trace the origin of the formula $F/W=a/g$ and to ascertain the consequences of its employment. The first extensive use of this formula appears to have been in the Ordnance College, Woolwich, by Professor Sir George Greenhill and the Instructors at the Royal Military Academy, Woolwich, C. S. Jackson and W. M. Roberts. Teachers of mathematics in public schools preparing candidates for the Army Entrance Examination found this formula quite sufficient for dealing with all the problems set in the various grades of mathematical paper because problems were almost invariably set in gravitational units only.

Greenhill's *Notes on Dynamics* was published by H.M. Stationery Office. These *Notes* mark a distinct advance in the teaching of dynamics as applied to practical problems. The book teems with examples which are in a class by themselves, and the treatment is brilliant throughout. Quoting from this book :—

" We begin Dynamics by considering motion in a straight line, as of a train on a straight railway, a shot in the bore of a gun, the piston of a steam engine, or water in a straight uniform pipe. This may be called the Dynamics of one dimension, and much use is made of it in the dynamical interpretation of facts familiar in everyday experience." 22

It is most remarkable that this book, so brilliantly original and so versatile and prolific in practical illustrations of every kind, should develop the subject of Dynamics without one single allusion to the term " mass ". It is also to be noted that the book contains no force or work problems in C.G.S. units.

Quoting from the section on *Moment of Inertia* :—

" Definition. The moment of inertia (M.I.) of a body about an axis is the sum of the product of the weight of each particle of the body and the square of the distance from the axis.

" Thus if m lb. is the weight of a particle at a distance r ft. from the axis, the M.I. of the particle is mr^2, in lb. ft^2, and of the whole body is Σmr^2 (lb. ft^2), ..." 23

The whole book shows a most careful and studious avoidance of the idea of mass. (See Quotation 59).

The *Notes on Dynamics* were followed by *A First Dynamics*, by C. S. Jackson and W. M. Roberts. This book contains an excellent collection of examples of all kinds, but hardly any at all on C.G.S. units. Quoting from the preface to the book :—

" There is still, perhaps, a danger that English text-books on Elementary Dynamics may become sharply divided into two classes, to which the dyslogistic epithets of ' academic ' and ' practical ' have been applied, the one for the public

school-boy and the candidate for University Scholarships, the other for the technical student and the engineer. We regret the existence of this distinction, and have tried to be neither 'academic' nor 'practical'." 24

"On account of its difficulty we have postponed the introduction of the notion of Mass until a later stage than has been usual." 25

Mass is not referred to until Chapter X on Newton and the Idea of Mass.

The following are quotations from Chapter X:—

"The student has realised that the notion of mass is not required for the dynamics of the phenomena of ordinary life on the earth's surface." 26

As has already been pointed out, mass is more fundamental to ordinary experience than weight.

"For scientific purposes the centimetre, gramme, second (C.G.S.) system is invariably used. Unfortunately, this point of view has not always been adopted." 27

"Much confusion has been caused by rejecting the scientific unit of mass, the gramme." 28

There are 105 Miscellaneous Examples at the end of the book. Not one of these deals with C.G.S. units.

The next stage in the argument consists in deducing the formula $P=kmf$ from the formula $P/W=f/g$ (see pp. 208, 209).

"Summary.—The same fundamental dynamical relation between force and acceleration has been expressed in different ways (for British units):

"(1) $P/W=f/g$. P and W in the same of any units of force, pounds, tons, etc.

"(2) $P=1/32\,mf$. m in pounds mass, P in pounds weight.

"(3) $P=mf$. P in pounds, m in slugs.

"(4) $P=mf$. P in poundals, m in pounds mass.

"It is worth noticing that, if for any purpose we wish to pass from (1) to *any* system of units involving mass in which the equation $P=mf$ is true, we have only to replace W by mg.

"For this reason we have no hesitation whatever in advising the student to adopt (1) as his standard dynamical equation for British units." 29

It will be observed that C.G.S. units are ignored. Again, formula (2) is chaotic—it should be rejected. Formula (1) rearranged is $P=Wf/g=mf$.

Thus, $P=mf$ is a relation which applies to any consistent system of units without any modification whatever.

Other text-books of dynamics have now appeared using the formula $P/W=a/g$. All these books reveal the same characteristics:—

1. They are written by mathematicians who have had little scientific or engineering training.

2. They either ignore C.G.S. units completely or the treatment is confined to a short paragraph and the examples are all together and entirely unrepresentative.
3. The idea of mass is either ignored completely or dealt with unsatisfactorily.
4. The treatment bears no relation whatever to the previous or future scientific training of the student.

Science text-books on *Mechanics* and *Hydrostatics* seldom emphasize the distinction between mass and weight at an early stage, or attend to the rigour of the treatment in problems on density and specific gravity. At a later stage, the formulae $F = ma$ and $F/W = a/g$ are both developed in order that the book may appeal to a wider market. This duplication of formulae and treatment is to be deplored.

§ 16. EXAMINATIONS

Examination syllabuses prescribe the subject matter to be taught and often indicate the type of treatment required. The careful setting of examination questions is of great importance and assistance in developing the subject in the right way. A student taught on historical lines should not expect questions on the history of the subject. Similarly, the student trained by the experimental method must not expect direct questions on dynamical experiments. He should be tested on the principles of the subject, and any excellence in the methods of teaching will be reflected in his grasp of those principles.

The early distinction between mass and weight, and the careful and correct use of these terms, should be emphasized in all examination syllabuses not only in physics but in mechanics (additional mathematics, applied mathematics, etc.).

Questions should be set in all systems of units, and the candidate should be expected to use all systems with equal facility, not only in "Science" but also in "Mechanics." This is extremely important, especially in the mechanics papers, where there is a tendency to set gravitational units only.

Some uniform system of notation and formulae is desirable.

Beam-balance weighings should be recorded as mass, and spring-balance readings as weight. The terms *mass* and *weight* should be used carefully and correctly in the working out of problems on density and specific gravity.

Forces in grams weight and pressures in grams weight per square centimetre should be rejected. The gram weight is unwanted and unnecessary, just as the dyne mass would be equally unwanted and unnecessary. These are not C.G.S. units, and inevitably lead to confusion. A pressure is expressed in dynes per square centimetre or as head of liquid.

Mass and weight should enter into formulae and equations naturally, so as to lead to dimensionally correct results.

Values of g should be given at the head of the paper and not at the end of the question. For approximate or rapid calculation $g = 32$ ft./sec.^{2} or, very roughly, 1000 cm./sec.^{2}, which is approximately 33 ft./sec.^{2}

APPENDIX 1

Attention is directed to quotations from the writings of members of the Committee responsible for the B.A. *Report* of 1907. It is probable that no member of this Committee agreed entirely with the whole of Clauses 19 and 20. Most of the members probably agreed with the first part of Clause 20 and would not favour Clause 19. The teachers of *Mathematics Applied to Military Engineering* would probably agree with Clauses 19 and 20 except the first part of Clause 20.

QUOTATIONS FROM ELEMENTARY TEXT-BOOKS

Oliver J. Lodge, *Elementary Mechanics, including Hydrostatics and Pneumatics* (1897.)

" . . . . it is hoped that students who use this manual will be able to master the elements of the science in such a way that they may rise from it to more advanced treatises, not only without having anything to unlearn, but with a very sound knowledge of principles." 30

" Experiments in Mechanics have a subordinate though most useful part in illustrating and emphasising the facts, but the author has no faith in making the establishment of principles depend on special experiments." 31

" The book, ... is intended to be not only an easy introduction to the subject, but, as far as it goes, a philosophical work." 32

" To recapitulate, then, mass means quantity of matter, and is measured by inertia." 33

The relation $F=ma$ is used. Examples illustrating the use of C.G.S., F.P.S., and gravitational units are given throughout the book.

W. D. Eggar, *Mechanics* (1905).

" The writer has found that Kinetics as well as Statics may be helped immensely by experimental work, and that the trouble involved in the preparation of lecture-experiments is amply repaid by the clearer notions gained by students on such points as acceleration, energy, and momentum." 34

" . . . . apart from the historical interest, Galileo's methods were so simple and accurate, his mathematics so sound and his arguments so lucid, that they ought to be more familiar. Experiments which resulted in the discovery of the laws of motion ought to be worth repeating." 35

" An experimental verification of Newton's Second Law has been given. To justify this rash act it has been necessary to state the law in Newton's own words, 'Change of motion *is proportional to* the impressed force', and the force has been measured in gravitational units. The dyne, and, if need be, the poundal, can then be defined, and recognised as not differing in kind from other forces of a more familiar type." 36

The relation $F = ma$ is used. All systems of units are illustrated, but gravitational units predominate in the examples. Experimental work is emphasized.

S. L. Loney, *Elements of Dynamics* (1899).

" The confusion is probably to a great extent caused by the fact that the word ' pound ' is used in two senses which are scientifically different; it is used to denote what we more properly call ' the mass of one pound ' and ' the weight of one pound '. " 37

The relation $P = mf$ is used. All systems of units occur in the examples.

Professor Arthur Morley, *Mechanics for Engineers* (1908).

" . . . . the gravitational system of units has been adopted in the English measures. A serious injustice is often done to this system in books on Mechanics by wrongly defining the pound unit of force as a variable quantity, thereby reducing the system to an irrational one. With proper premises the gravitational system is just as rational as that in which the ' poundal ' is adopted as the unit of force, whilst it may be pointed out that the use of the latter system is practically confined to certain text-books and examination papers, and does not enter into any engineering work. Teachers of Engineering often find that students who are learning Mechanics by use of the ' poundal ' system fail to apply the principles to engineering problems stated in the only units which are used in such cases—the gravitational units. The use of the dual system is certainly confusing to the student, and in addition necessitates much time being spent on the re-explanation of principles, which might otherwise be devoted to more technical work." 38

The relation $F = mf$ is used. Gravitational units are used throughout except Example 10, p. 47. A collection of examples at the end of the book taken from examination questions set for Inter. B.Sc. London (Engineering), A.M.I.C.E., and Board of Education Applied Mechanics does not contain a question relating to C.G.S. or F.P.S. units.

The second part of Quotation 38 requires examination. It illustrates the undesirability of teaching one set of units to the exclusion of all others. Professor Morley complains of the poundal system because he is preoccupied with the gravitational system. His students, who may have been equally preoccupied with the poundal system, may complain of the gravitational system. The beginner should be able to use all systems of units with equal facility. A student who can work in only one system of units is less than half educated. Again, the mechanical or civil engineer cannot expect all early training to be conducted exclusively according to his taste if this runs counter to the requirements of others.

J. W. Landon, *Elementary Dynamics* (1920). (A text-book for engineers.)

" Quite commonly in elementary text-books, the second law of motion is at first summed up in the form force = mass × acceleration, and the student is then given a number of examples to work out, most of which consist in substituting

numbers in a formula. This very successfully disguises the true meaning of momentum, and the extraordinary generality of the second law of motion." . 39

"At first sight the remedy would appear to lie in teaching dynamics experimentally, but the author's experience is that this is not so for the majority of the students. The phenomena of everyday life provide innumerable qualitative experiments, and to most students quantitative laboratory experiments in dynamics are neither interesting nor convincing." 40

"In working examples the absolute unit of force has generally been adopted, and, where applicable, the answers have been reduced to units of weight. It matters little, in the author's opinion, whether absolute or gravitational units are used, so long as mass is not defined as weight divided by the acceleration due to gravity. To say that the engineer's unit of mass is 32·2 lbs. is almost to suggest that he is rather lacking in intelligence, and cannot be expected to understand the difference between equality and proportionality. If weight is introduced in the early conception of mass, the student's conception of mass is extremely vague, and his conception of momentum as a physical quantity is even more vague or erroneous. A student who cannot understand the difference in the two units of force, and who has merely to rely on formulae expressed in one particular set of units, is not likely to get any knowledge of dynamics which will be of real use to him." 41

"It is very important to distinguish between MASS and WEIGHT." . . . 42

"As we have previously stated, mass is merely the quantity of matter, and is a *scalar* quantity; *weight*, on the other hand, is a *force*, and is a *vector* quantity (its direction being always towards the centre of mass of the earth)." . . . 43

Force is defined as rate of change of momentum. All systems of units are discussed, but of the 100 miscellaneous examples at the end of the book, there is not one in C.G.S. units.

Quotation 39 states that the formula $P=ma$ is often applied mechanically and disguises the true meaning of momentum. Mr. Landon prefers the relation between force and change of momentum to $P=ma$. Will it not happen that if the relation $P=ma$ is ignored, the definition of mass will be successfully disguised and momentum defined in terms of this disguised quantity? Let a student be quite clear on every aspect of the relation $P=ma$ terms, units, experiments, simple illustrations. Let him see pictorial and concrete examples of the application of this important and fundamental relation. Let him mentally work numerous simple direct calculations involving this formula in all systems of units until the use of the relation is automatic and purely mechanical. Then more difficult problems involving thought. The student does not have to think how to dress each morning or how to walk upstairs at night. These quite complicated operations would become embarrassing if thought about—they must be performed automatically and subconsciously. The formula $P=ma$ must be applied with equal facility and readiness.

After this, force may be studied as time rate of change of momentum or space rate of change of kinetic energy. Some problems immediately involve one relation and other problems involve others.

The key to a complete understanding of all the force relations is a thorough comprehension of the idea of mass as embodied in the formula $P = ma$.

There is no reason why the best features of all these books should not be incorporated in all future text-books of elementary dynamics. It is most desirable that all beginners should be trained in the same way. The treatment should be rigorous and philosophical, and can be made interesting by frequent allusion to the history of the subject and the experiments of the founders of its principles—for example, Galileo and Newton (see Quotations 35 and 36). The book should teem with examples of the most varied type—easy, direct, numerical work involving round numbers worked out mentally, more difficult numerical work, graphical work, general problems, data of dynamical experiments, practice in the use of various systems of units. The same treatment, symbols, formulae and equations should be used for all units. Chaotic, mixed, unwanted, unnecessary and irrational units such as the gram weight or gram weight per square centimetre should receive critical examination but should not be used.

Remarks on mass and weight by engineers

Machinery and Millwork, by W. J. M. Rankine (1876).

"If by the unit of force is understood the weight of a certain standard, such as the avoirdupois pound, then the mass of that standard is $1 \div g$, and the unit of mass is g times the mass of the standard; and this is the most convenient system for calculations connected with mechanical engineering, and is, therefore, followed in the present work." 44

"But if we take for the unit of mass the mass of the standard itself, then the unit of force is the absolute unit; for g is the velocity which a body's own weight, acting unbalanced, impresses on it in a second. This is the system employed in many scientific writings, and in particular in Thomson and Tait's *Natural Philosophy*. It has great advantages in a scientific point of view; but its use in calculations for practical purposes would be inconvenient, because of the prevailing custom of expressing forces in terms of the standard of weight." 45

Applied Mechanics, by John Perry (1899).

"When a body is in motion, it possesses kinetic energy equal to half its mass (its weight in London in pounds divided by 32·2 is its inertia, which is usually, but we think unwisely, called its mass) multiplied by the square of its velocity in feet per second." 46

Calculus for Engineers, by John Perry (1897).

"To satisfy the College men who teach engineers I would say that 'The unit of Mass is that mass on which the force of 1 lb. produces an acceleration of 1 ft. per sec. per sec." 47

"We have no name for unit of mass, the engineer never has to speak of the inertia of a body by itself." 48

Steam Power, by W. E. Dalby.

In the chapter on the balancing of reciprocating masses the formulae $F=MA$ poundals and $F=MA/g$ lbs. wt., $M=$ mass in pounds are given. 49

The Steam and other Heat Engines, by J. A. Ewing.

"Let M be the mass of the piston, piston rod, and cross-head in pounds, and a its acceleration at any instant in feet per second per second, the force required to accelerate it is Ma/g, in pounds weight . . . ." 50

Theory of Machines, by Louis Toft and A. T. J. Kersey.

The mass of a body is a measure of the quantity of matter in it, and its value in engineer's units is the ratio of the weight of the body in pounds, at any part of the earth's surface, to the value in feet per second per second of g, the acceleration of a body falling freely under gravity at the same place. The weight W lb of a body is the pull of the earth on the body, and varies slightly with the locality, but the ratio W/g is constant.

The unit of mass is then the quantity of matter in a body weighing g lbs." 51

Later the formula $F=Ma$ is given.

The Quotations 44 to 51 occur in well-known text-books of engineering. Not one of these writers avoids the use of the term mass, but all use the pound weight as the unit of force. The use of the pound as the unit of mass as well as the unit of force is the cause of all the confusion and difficulty of beginners. Quotations 47 and 48 avoid this inconsistency of units, but reveal the astounding fact that engineers habitually use a unit without giving it a name. It must also be remembered that although mechanical engineers may not speak frequently "of the inertia of a body by itself" there are aeronautical, civil, electrical, marine and other types of engineer, quite apart from those who are not engineers. All students receive their early training together, and the subjects of physics and mechanics must be taught not in terms of engineering but in the best interests of science and engineering.

If m is mass in pounds, $F=ma/g$ is true numerically but not dimensionally.

If W is weight in pounds, $F=Wa/g$ is true numerically and dimensionally. It is true of a body at a place where it has little or no weight: it is true of bodies everywhere. It is true of objects having no weight. It is the formula $F=ma$, but m is expressed as the ratio of two variables. Why is the ratio of two variable quantities W and g used in preference to a single constant quantity m?

More than one reason for this may be given:—

(1) W is introduced for popular convenience.

Surely, precise scientific expression should not be sacrificed for popular convenience.

(2) W is introduced because engineers think of force in pounds weight.

All the more reason for expressing mass in terms of a unit consistent with force in pounds weight. With force in pounds weight, mass should be in slugs.

(3) W is used to represent pounds mass.

This procedure is really bad and dimensionally incorrect. The use of chaotic units in a chaotic formula should be rigorously rejected. If a problem involving pounds mass as well as pounds weight arises, one of these units must be changed before substitution is made in any rational formula.

Remarks on mass and weight by mathematicians

Horace Lamb, *Dynamics* (1914).

In Chapter II there are sections headed *Gravitational Units* and the *Absolute System of Dynamics*. Both equations $P=ma$ and $P=Wa/g$ are given.

" It is now usual to designate inertia, when regarded as a measurable quality, by the term ' mass '; the mass of the body on the present reckoning is therefore W/g." 52

" It is scarcely necessary to insist on the special sense which the word ' force ' has come to bear in Mechanics. It is perhaps unfortunate that some more technical term was not introduced instead of a word which, in popular language, has so many different meanings. The usage is, however, long established and must be accepted." 53

" There is not the same agreement, although there has been much controversy, as to the use of the word ' weight '. In ordinary language this is employed in a great variety of senses. Thus it may mean the actual statical *pressure* which a body exerts on whatever is supporting it, as when we speak of the ' weight ' of a burden; it may mean the *ratio* which this pressure bears to that exerted by a pound or a ton; it is often used virtually in the sense of *mass*, as when we refer to the weight of a projectile; when, again, we speak of the ' weight ' of a blow the idea is (vaguely) that of *momentum*. The one sense in which the word is *never* used in popular language is that of the gravitational attraction on a body. This is, of course, equal to the statical pressure above referred to, but it is not identical with it; it is a force exerted *on* a body, not *by* it. Unfortunately, this new and alien sense is precisely that which some writers of eminence have sought to attach exclusively to the word ' weight ' in Mechanics. In the author's opinion it is best not to attempt to specialize altogether the meaning of so familiar a word, but to use it freely in whatever sense may be convenient, whenever there is no risk of misunderstanding. When there is danger of confusion, some other term, such as ' mass ' or ' gravity ', may be employed to indicate precisely the sense which it is wished to convey." 54

" In the rest of this treatise we follow the absolute system, as by far the most convenient for general application." 55

A. E. H. Love, *Theoretical Mechanics* (1906).

" It is clear that the definition of mass by means of mutual action is more general and more fundamental than that by means of weighing. We shall show . . . . that the determination of masses by weighing is a particular case of the determination by means of mutual action.

" Since we are accustomed to estimate the *quantity of matter* in a body by weighing the body, it is customary to state that the quantity of matter in a body is equal to the mass of the body." 56

" Thus the mass of the body provides a measure of its *inertia*." . . . 57

Alfred George Greenhill, *Hydrostatics* (1894).

" With the Gravitational Unit of Force, the weight of a body is at once the measure of the quantity of matter in the body, and also the force with which it is apparently attracted by the Earth; and the word Weight may be used in either sense without ambiguity or confusion, when dealing with hydrostatic problems on the surface of the Earth." 58

Alfred George Greenhill, *Notes on Dynamics* (1908).

" We use the word Weight advisedly, so as to agree with the precise language of the successive Acts of Parliament.

" The weight (poids, pondus) of a body is the quantity determined by weighing; to weigh (poise) the body, it is placed on the one of the scales of a balance, and equilibrated with certain lumps of metal in the other scale, called weights; and the sum of these weights is called the weight of the body." . . 59

" A force of F pounds acting on a weight of W lb. will give it acceleration a f/s^2, such that $a/g = F/W$." 60

Professor E. T. Whittaker, *Analytical Dynamics* (1937).

" If any set of mutually connected particles are in motion, the acceleration with which any one particle moves is the resultant of the acceleration with which it would move if perfectly free, and acceleration directed along the lines joining it to the other particles which constrain its motion. Moreover, to the several particles A, B, C, ..., numbers m_A, m_B, m_C, ... can be assigned, such that the acceleration along AB due to the influence of B on A is to the acceleration along BA due to the influence of A on B in the ratio $m_B : m_A$. The ratios of these numbers m_A, m_B, ... are invariable physical constants of the particles.

" The evidence for the truth of this statement is to be found in the universal agreement of the calculations based on it, ... with the results of observation.

" It will be noticed that only the *ratios* of the numbers m_A, m_B, m_C, ... are determined by the law; it is convenient to take some definite particle A as a standard, calling it the *unit of mass*, and then to call the numbers m_B/m_A, m_C/m_A, ... the *masses* of the other particles m_B, m_C, ...

" The mass of the compound particle formed by uniting two or more particles is found to be equal to the sum of the masses of the separate particles. Owing

to this additive property of mass, we can speak of the mass of a finite body of any size or shape; and it will be convenient to take as our *unit of mass* the mass of the 1/1000th part of a piece of platinum known as the *standard kilogramme*; this unit will be called a *gramme*, and the number representing the ratio of the mass of any other body to this unit mass is called the *mass of the body in grammes*." 61

" In general, if an acceleration represented by a vector f is induced in a particle of mass m by any agency, the vector mf is called the *force* due to this cause acting on the particle; and the resultant of all the forces due to various agencies is called the *total force acting on the particle*." 62

Professor Karl Pearson, *The Grammar of Science* (Part 1, 1911).

" The proper measure of mass is found to be a ratio of mutual accelerations, and force is seen to be a certain convenient measure of motion, and not its cause. The customary definitions of mass and force, as well as the Newtonian statement of the laws of motion, are shown to abound in metaphysical obscurities." . . 63

Quotation 54 refers to the many popular meanings of the word " weight ". In a beginner's science course, the earliest opportunity should be taken to explain the scientific meaning of this term. The beginner is quite familiar with the *idea* of mass but not with the *word*. It is not difficult to put this right, but the difficulty of correction becomes greater and greater if the young pupil is allowed to establish the habit of using incorrect terms in a scientific problem. This also gives the opportunity of discussing the more difficult idea of " weight ", the gravitational attraction of the earth. This word must not be used in any other sense. The mind of the young beginner is easily moulded. Bad habits must be eradicated at once and the process of correction must be immediate. Any delay increases the difficulty of correction and the possibilities of confusion. The distinction between mass and weight must be discussed at the very beginning. Once these terms have been explained, they must receive careful and persistent attention.

Quotation 58 studiously avoids all reference to mass. The word does not occur anywhere in *Notes on Dynamics*. The word " weight " is used for " mass " throughout the book. The book contains no force or work problems in C.G.S. or F.P.S. units. It is probable that Prof. Greenhill originated the use of the formula $F/W = a/g$. This formula involves a different unit of weight in each place.

Quotation 61 is a statement that could scarcely be improved upon. It is simple, lucid, and philosophically rigorous. It is as easy to realize that $m_1/m_2 = f_1/f_2$ as $P/W = a/g$, but the first relation is a ratio of concrete quantities and is true universally, whereas the second relation involves the ratio of two forces in different directions, and these forces must act on the same body at the same place relative to the earth.

Quotation 63 indicates the futility of philosophical discussion, at any rate for beginners. Quoting from J. B. Stallo's *Concepts of Modern Physics*:—

" Mass and motion being mutually inconvertible, mass is absolutely inert." 64

" Once more, then, science is in irreconcilable conflict with one of the fundamental postulates of the mechanical theory. Action at a distance, the impossibility of which the theory is constrained to assert, proves to be an ultimate fact inexplicable on the principles of impact and pressure of bodies in immediate contact. And this fact is the foundation of the most magnificent theoretical structure which science has ever erected—a foundation deepening with every new reach of our telescopic vision, and broadening with every further stretch of mathematical analysis." 65

Action at a distance, variable mass, absolute space and time, the real object, and other similar problems are not suitable for elementary discussion.

Remarks on mass and weight by physicists

W. Thomson and P. G. Tait, *Natural Philosophy* (1879).

" The Quantity of Matter in a body, or, as we now call it, the Mass of a body, is proportional, according to Newton, to the Volume and the Density conjointly. In reality, the definition gives us the meaning of density rather than of mass; . . . ." 66

" Newton further states that a practical measure of the mass of a body is its Weight. His experiments on pendulums . . . ." 67

J. Clerk Maxwell, *Matter and Motion* (1882).

The Chapter on Force is particularly illuminating. See also Chapter IV, Maxwell's *Heat*.

" . . . . Of these three methods, that of weighing depends on the attraction between the acid and the earth, that of measuring depends on the volume which the acid occupies, and that of titration depends on its power of combining with potash." 68

" In abstract dynamics, however, matter is considered under no other aspect than as that which can have its motion changed by the application of force. Hence any two bodies are of equal mass if equal forces applied to these bodies produce, in equal times, equal changes of velocity. This is the only definition of equal masses which can be admitted in dynamics, and it is applicable to all material bodies, whatever they may be made of." 69

J. H. Poynting and J. J. Thomson, *Properties of Matter* (1905).

" This constancy of acceleration under a given force is expressed by saying that the mass of the body is constant." 70

" Further experiment shows that the acceleration of a given body is proportional to the force acting on it." 71

"The masses of bodies are proportional to the forces producing equal accelerations in them." 72

"But for scientific purposes all over the world the unit of mass is the gramme, the one-thousandth part of the mass of the piece of platinum-iridium called the Kilogramme-International, which is kept at Paris." 73

Henri Poincaré, *Science and Method*, translated by Francis Maitland. No date (after 1913).

Poincaré has made contributions to philosophy, mathematics, astronomy and physics. The section in the book on *Mechanics and Radium* is of interest. The following quotation is from his remarks on the *New Mechanics and Astronomy*. He discusses real and fictitious mass, but the only bearing on the present discussion is the fact that a body may have no weight, but it may have mass, and mass varies with velocity, as in the case of a rapidly moving charge.

"Mass may be defined in two ways—firstly, as the quotient of the force by the acceleration, the true definition of mass, which is the measure of the body's inertia; and secondly, as the attraction exercised by the body upon a foreign body, by virtue of Newton's law. We have, therefore, to distinguish between mass, the coefficient of inertia, and mass, the coefficient of attraction." . . 74

See also Routh's *Analytical Statics*, Vol. II.

Quotations 66 to 74 indicate remarkable uniformity in point of view and treatment. The physicist uses $F=ma$, a formula which may be applied without modification to any system of units.

John Cox, *Mechanics* (1904).

The treatment in this book approximates closely to that advocated in this paper. It is not altogether an elementary text-book but one for university classes. All systems of units are discussed and included in the examples. Beginners require more numerous examples of great diversity of type and difficulty.

" . . . . $P=ma$.

"The simplest way of solving dynamical problems is to use this formula in conjunction with the Kinematical formulae . . . ." 75

The following quotations are from the Preface:—

"It is a common complaint that though the principles of Mechanics are the simplest and the earliest to be discovered in the whole range of Science, and, moreover, are directly illustrated in almost every act of our lives, more difficulty is found in giving beginners a real grip of them than with any other branch of Physics.

"This I attribute largely to the way in which the text-books deal with the subject. The student usually opens the book upon a chapter in which such leading concepts as matter, force, mass, particle, rigid body, smooth body are treated in definitions of a line or two each, before he sees any reason for their

introduction at all. He is probably warned that philosophers are not agreed about the nature of matter; that motion is purely relative; that force is a misleading idea borrowed from our muscular sensations and better got rid of; and that no such things as mathematical particles, rigid bodies and smooth bodies exist in nature. He naturally concludes that Mechanics is an abstruse subject having nothing to do with realities or common sense." 76

"The second chapter plunges him into the mathematical study of motion in the abstract. . . . . To his previous confusion he adds the conviction that this is only another branch of the pure mathematics he has hitherto found so little use for." 77

"At last there is a chapter on the Laws of Motion, so inadequately treated that he often ends by believing that they were made up by Sir Isaac Newton, the author, so far as he is aware, of the whole subject. The rest of the book is too often merely geometrical and trigonometrical gymnastics." 78

"In recent years many text-book writers have attempted to break away from this mischievous tradition. Some have tried to rewrite the whole subject from the latest point of view of Energetics. But this is surely to begin at the wrong end. According to the biologists the bodily development of the individual is an epitome of the development of the race. Is not this a hint that the historical method is the natural way of attacking a subject of study? Others have sought to discard the idea of force, and speak only of mass-accelerations. *Naturam expellas furca.* It is rarely indeed that they manage twenty pages without getting back to the old point of view. With proper caution the use of this concept is as valuable as it is historically right and inevitable. Still others have set the student to rediscover the subject for himself by experiment. But this wastes too much time on mere manipulation, and leaves the student's knowledge in mid-air, unrelated to all that has gone before him in the course of actual discovery. It seems a pity that he should close the book without a glimmering of personal interest in his predecessors, the great investigators, and forego the insight into philosophic and scientific method which a study of the development of mechanics evokes insensibly and unawares." 79

"After learning and teaching Mechanics for ten years on the traditional system described above, I was called on, as a lecturer under the Cambridge University Extension Scheme, to explain the principles to audiences without any previous mathematical training, but often composed of engineers, plumbers, and other workmen who had derived excellent practical notions on the subject from their experience. Obliged thus to recast the subject in my own mind, I found it possible to present all the main principles with the aid of ordinary arithmetic and the simplest geometrical diagrams. At this stage Sir Robert Ball's admirable lectures on *Experimental Mechanics* gave me great assistance. My experience with these popular audiences reacted with advantage on my

teaching with classes in the university, and fired me with the ambition to write a text-book on Mechanics. But a sight of Sir Oliver Lodge's excellent *Mechanics* in Chambers' series put an end to this wish for a time. Some ten years ago I stumbled on the first German edition of Professor Mach's *Die Mechanik in ihre Entwickelung*. I am ashamed to say that this fascinating book was my first introduction to the historical development of a subject I had taught so long. Since then my teaching has been based more and more on the lines laid down by Mach, and as I have found it impossible to induce ordinary students to read the original, even when translated, I recurred to the idea of writing a text-book which should yet be based on Mach's method." 80

" Until Mechanics is clad in its historical flesh and blood, it will remain the dull and tiresome subject that has convinced so many generations of students that an abysmal gulf separates theory from practice." 81

The following works may be consulted for further historical detail:—

Dr. Charles Singer, *Studies in the History and Method of Science*.

Prof. Wolf, *A History of Science, Technology, and Philosophy*.

APPENDIX 2

This paper was written before the author was aware of a *Report on the Teaching of Mechanics in Schools*, prepared for the Mathematical Association (1930, published by G. Bell & Sons, Ltd.).

The Sub-Committee which prepared the Report was composed as follows:—

W. J. Dobbs. Miss D. R. Smith.
W. C. Fletcher. Miss L. M. Swain.
C. J. A. Trimble.

A. Robson, *Secretary*.
C. O. Tuckey, *Chairman*.

The Report is a valuable contribution to the teaching of Mechanics. The parts of it relating to the present discussion occur chiefly in paragraphs 1, 2, 3 and 6. Unfortunately, the assistance which science and mathematical masters might give to each other is ignored. No attempt is made to explore possibilities of co-operation (see Appendix 4), and the report deals primarily with the teaching of mechanics as a part of elementary mathematics for the School Certificate Examination or for non-specialists.

" Mechanics . . . . is sometimes included in science and sometimes in mathematics. If it is included in science as part of physics, it is generally done very slightly, the main attention being given to heat, light and electricity. The reason for this is probably that the science master finds mechanics less suitable than the other subjects for his characteristic method of teaching, with its constant appeal to experiment." 82

" In mechanics . . . it is to experience rather than to experiment that constant appeal must be made, though it is not denied that experiment may be of great value. Moreover, the very large amount of written example work desirable is more consonant with the atmosphere of the mathematical class-room. Hence it is more usual to take mechanics as part of mathematics." 83

" Probably the ideal arrangement would be that the same man should teach the same group of boys in both mathematics and physics; he would then be able to use experiment without the difficulties felt by the mathematical master, and at the same time to do the bulk of the work in mechanics in the mathematical hours.

" This arrangement, however, is not often found to be possible, and, as things stand, this Committee has no hesitation in endorsing the correctness of the usual procedure by which mechanics is included as part of mathematics." . 84

These views are narrow and short-sighted. Mechanics is taught both by science and mathematical masters. Who teaches it, and whether it forms part of mathematics, or physics, or both, is not relevant unless the principles of mechanics are different in the different subjects. It is not for the science master or his mathematical colleague to prescribe for the other how much experiment, experience, geometry, trigonometry, etc., he should bring into his teaching. Nor should there be controversy as to right of way. The fullest co-operation between masters and the closest correlation of subject matter and methods are essential in the best interests of the student. What is really unfortunate is that the spirit of Quotation 84 dominates the recommendations of the report.

I am strongly of the opinion that statics and dynamics should form part of mathematics and that statics, dynamics, and hydrostatics should form part of physics. The same treatment, notation and formulae of fundamental principles should be followed by both mathematical and science masters.

" The bulk, therefore, of this *Report* is devoted to a discussion of the best methods of carrying through a comparatively limited pre-certificate course of mechanics, though the needs of specialists and other more advanced students have also been considered." 85

The needs of the non-specialist are fully provided for in a course of General Science. These boys will not as a rule proceed to the stage at which trigonometry is part of the mathematical course.

" Mathematicians, scientists, engineers, and others need not be separated for teaching purposes on any other basis than that of ability, unless it happens that, e. g., specialists in physics and chemistry have insufficient time to work with the mathematical specialists." 86

It will be agreed that all boys who study mechanics as part of mathematics will receive the same training together throughout the school.

Paragraphs 2 and 3

These paragraphs deal with experience and experiment.

" The purpose of observation or experiment should be to enable boys to realise how the thing works. Quantitative results are relatively unimportant." 87

" Perhaps the whole may be summed up by saying that, while quantitative work may be dispensed with, effective steps can and must be taken to meet the weakness of geometrical imagination, which is one of the great stumbling blocks." 88

Many will neither entirely agree nor entirely disagree with Quotations 87 and 88. I should regard Galileo's experiments on uniform acceleration as important although quantitative, and if I taught mechanics as part of mathematics there would be many uses for the experimentally determined values of s and t. I regard the evaluation of g by all manner of different methods as an extravagant use of valuable time.

Paragraph 6

" This section of the report deals with the most controversial parts of the subject: how and when to introduce the idea of mass; whether or not to use an absolute system of units; if such a system is used, whether it should be based on the unit of mass or the unit of force.

" These and allied questions are the questions most hotly debated among teachers of mechanics. The sub-committee preparing this report has given a great deal of time to their consideration, and if complete unanimity has not been reached, at least the opposing views were found at the end to differ much less widely than at the beginning of the discussions." 89

The report contains an excellent account of the controversial points. There was complete agreement of the Committee on two matters:—

6.1. " *Force and rate of change of momentum should be thought of by the learner as different things.*" 90

6.2. " *The double meaning of the word ' pound ' should be introduced early.*" 91

" Much of the difficulty, real or supposed, of dealing with mass would be abolished if teachers would adopt a very simple precaution.

" The word ' pound ' is used in mechanics in at least two senses, if not by pure mathematicians certainly by engineers. This may be regrettable, but we cannot change it. The pedagogic error consists in starting with the one sense (force) without warning that there is another (mass), and that it is the other which is fundamental. The further the subject is pursued with the one meaning of the term, the more deeply rooted does that meaning become, and the greater the difficulty in making a change. In statics we measure forces in pounds and sow the seed of trouble. Frequently we continue the practice on starting dynamics, and the association of pound with force gains strength. Still further,

a new term which is coming into use, the ' second-pound ' as a unit of impulse or momentum, also tends to fix the notion of pound as force if a careless treatment is adopted. The same objection does not apply to sec. lb. wt.

" After all this we attempt a *volte face* and teach that a pound is not properly a force at all, but something quite different—a mass." 92

I should like to have quoted the whole of this paragraph. It very ably exposes the inevitable confusion arising from not distinguishing between mass and weight.

It will be observed that the student of mechanics has a further treat in store for him—the second-pound as a unit not only of impulse but also of momentum.

" While we do not need ' mass ' in statics and while long established custom sanctions the use of ' pound ' as a unit of force, it is strongly urged that from the outset the other meaning of ' pound ' should be kept in mind so that, when occasion arises for the use of that meaning, boys' minds should not be unprepared and resistant." 93

Surely this can only mean that mass should be considered from the outset. It is stated in the report that complete agreement was reached by the Committee with regard to 6·2. A majority of the Committee were in agreement with 6.3, 6.4 and 6.5, but 6.3 is inconsistent with 6.2.

6.3. " *In teaching the average beginner the fundamental equation of dynamics should first be obtained and used in the form $P/W=f/g$, the consideration of mass being postponed for a time.*" 94

" Dynamics deals with the production of motion in matter by force. The property of matter which concerns us here is mass." 95

Quotations 94 and 95 seem to suggest that, in the case of a beginner, the fundamental dynamical relation (which deals with mass) should be dealt with by postponing the consideration of mass.

" As set out in detail later, it is less difficult than is sometimes supposed to explain how mass is measured, but the difficulty is for the beginner to acquire any feeling of the significance of the mass as distinct from the weight of the same lump of matter; the same standard pound provides the unit in each case, either by its mass or by its weight, and the two numerical measures are therefore the same." 96

" Fortunately it is possible to make the practical question involved—the question of the motion produced by a given force—essentially simpler for the beginner by the postponement of the consideration of mass, and the use of gravitational instead of absolute units." 97

" In this method the fundamental equation of the subject is not used in the form $P=mf$ . . . . Instead it is obtained in the form $P/W=f/g$.

" This last fact affords the additional advantage that the forces P, W, and also the accelerations f, g, may be measured in any units we please." 98

In connection with Quotation 94 it may be pointed out that there is no difficulty with regard to a beginner using the idea of mass, particularly if the distinction between mass and weight has been dealt with at an early stage in the science course. If this has been done the use of $P/W=a/g$ is likely to lead the student into utter confusion. The application of this formula to gravitational units cannot fail to give correct results, particularly and only in chaotic units. The user of this formula not only condones the chaotic units pound weight and pound mass, but he proceeds to inject chaos into rational systems of units. This formula is a complete failure in connection with C.G.S., F.P.S. and the true engineers' system of units, in which the unit of mass is the slug. It will be observed that in order to introduce this formula, every idea of rational scientific procedure is sacrificed. Still further, the whole scheme must be scrapped in the end, so that at some later stage the student must change over to rational methods. The user of this formula proceeds to use it for his own purposes and, having created a condition of chaos so far as others are concerned, these unfortunate others and the pupil himself are left to extricate order out of confusion.

To begin with, this formula involves:—

1. Postponement of the idea of mass until a rational system of units comes into use.
2. The use of chaotic units exclusively.
3. The use of chaotic formulae.
4. The introduction of the idea of weight into problems in which weight is not directly involved.
5. A change-over to a rational system after training the student in chaotic units and formulae.
6. Complications with regard to quantities such as moment of inertia (see Quotations 14 and 15).

The remedy for all this confusion is very simple:—

1. Deal with mass early.
2. Call the engineers' unit of mass a " slug ".
3. Never use chaotic units or formulae. If chaotic units arise in a problem, change them to rational units.
4. Work problems in all systems of units.
5. Use $P=ma$, from which unit force produces unit acceleration in unit mass.
6. Co-operative effort by mathematical and science masters, so that there is complete uniformity of treatment and it is not necessary to change any of the earlier ideas.

6.4. *Mass.*

The report indicates an inferiority complex with regard to the successful explanation of mass to beginners. Teachers of mechanics should find that this has already been carried out by the science master unless the latter finds that all his early hard work is regularly shattered by the use of $P/W=a/g$.

" The difficulty that arises from the use of the pound and gram both as units of mass and units of force is inevitable, but is not so great as it is sometimes thought to be." 99

Why create difficulties by introducing unwanted units? If a student is trained to use rational units rationally there are no difficulties, even in the training of the student.

6.5. *The change from the habitual use of gravitational units to that of absolute units.*

" Those who commence with the use of gravitational units should, if they continue their studies sufficiently far, change over to the habitual use of absolute units (except for working certain types of numerical examples)." 100

" It is recommended that the change should be effected in two stages: a preliminary stage in which absolute units are introduced and used for the first time while gravitational units still remain dominant, and a final stage in which absolute units become habitual and gravitational units are merely reserved for problems to which they are specially suited." 101

Comment seems superfluous. The procedure here advocated is a fitting climax to the use of $P/W = a/g$.

6.6. *The " Absolute " Theory.*

A minority of the Committee were in agreement with 6.6.

" The last three sections represent the views of what may be called the gravitational school; this section attempts to state once more the views of the other school, dominant at the end of the last century, but now gone out of fashion so far as school-books are concerned." 102

" The essential points in the views here set forth are the introduction from the outset of the term mass in its simple sense and the correct use of the word pound." 103

" . . . . shall we use absolute units from the start or shall we use gravitational units at first and only later, and gradually, introduce the others? " 104

" It may be noted that mass is a more primitive thing than force in the sense that it is much easier to define a standard of mass than a standard of force; . . . ." 105

" . . . . dynamics is essentially the study of the relations of three entities . . . force, matter, and motion, and any treatment which ignores or obscures one of the three is essentially defective." 106

May I add:—

1. Use all units, but all formulae and working should require the conversion of chaotic or irrational units to a rational system before introduction into dynamical relations.

2. Use $P = ma$ to define the units of force or mass in any rational system of units.

3. The early distinction between mass and weight should be emphasized by the science master, and the mathematical master should continue a treatment already begun. The science master will also continue at a later stage, and there should be no modification in the method of working at any stage of the student's career.

4. Reject such units as gram weight or grams weight per square centimetre.

5. Call the engineers' unit of mass a slug.

APPENDIX 3

"Weight" meaning "mass"

There must be something radically wrong with any system of education which leads to a necessary distinction and difference between science and popular science. Similarly, the popular incorrect use of the word *weight* for *mass* is an indication of defective education. The scientific worker has been far too tolerant of the superior attitude of those who criticize a split infinitive but connive at and condone the inaccurate use of common words. In my opinion it is worth while making an attempt to correct popular speech and thought. This could be done in a single generation. The wording of Acts of Parliament should be corrected, and not tolerated. Teachers of English and mathematics should begin early to correct this slovenliness and inaccuracy of speech. Science masters do not as a rule teach *all* students, whereas teachers of English and mathematics not only influence the whole rising generation, but they can make that influence felt early and before the science master gets a chance.

I have expressed the opinion that the misuse of certain words could be corrected in a single generation. Lord Woolton could go far in this direction by suitable wording of food edicts. If the cigarettes called "Weights" had been called "Masses" the public might become curious, but if the makers of these cigarettes would change the name and state the reason for the change on the packet or in newspaper publicity, this would be a public service. Similarly, if sweets, sugar, meat, etc., were sold by mass instead of weight, the word mass would rapidly take the place of weight, and with its correct meaning.

What is implied in Paragraph 6.4, Appendix 2, "the teaching of mass being postponed for a time"?

There are two unsatisfactory suggestions here:—

1. A student is to be allowed to use the word "weight" for "mass" until a later stage of the work is reached when mass calls for consideration. Does this not encourage inaccuracy and slovenliness of thought and speech?

2. A student beginning dynamics is to avoid the consideration of mass. This implies that early studies in dynamics should avoid the consideration of dynamics and seek some alternative method of study in which fundamentals are

side-tracked. This is not dynamics but subterfuge. It leads only to unfailing accuracy in the substitution of numbers in a formula. It would be far better to defer the teaching of dynamics until a student is capable of studying the subject in a rigorous and proper manner (see Quotations 38 and 41).

APPENDIX 4

The Report of Sir J. J. Thomson's Committee on Natural Science in Education (1918) contains the following:—

" Much stress has been laid in the evidence before us on the need for correlation of school teaching in Mathematics and Natural Science. There is evidently a strong feeling, both among school teachers and among those who have to deal with the products of the secondary schools at the universities and in the industries, that both subjects would gain if they were taught with more reference to each other, and, so far as possible, by the same teachers. At present it is easier to get a science master who is able to teach elementary mathematics than a mathematical master with a corresponding knowledge of science; but few schools can spare a science master for any part of the mathematical work. The need for co-operation is not felt only on the side of the natural sciences; the Committee of the Mathematical Association express the views that effective correlation between the two subjects is rare and is not increasing, that an ideal arrangement would assign the teaching of mathematics and of physics largely to the same masters, and that the education of teachers of mathematics should be conducted with this end in view." 107

" Any want of co-ordination between Science and Mathematics has a particularly unfortunate effect on the teaching of mechanics. This subject is rightly regarded as of the greatest importance. It is the basis of most parts of physics, and most schools make some provision for teaching it, either as a branch of Mathematics or as an experimental subject, or from both points of view; but the evidence we have received indicates that in a considerable number of schools the results obtained are far from satisfactory. This is much to be regretted, for the subject is especially suitable for training the student to use his mathematics and to apply it to the problems which he may have to face. The power of doing this is a very valuable asset for those who will become engineers or be occupied with other applications of Science to industry; while, apart from this aspect, it affords a mental training of the highest importance and gives to many an interest in mathematics and a grip of its principles which, without it, they would never acquire." 108

I am of the opinion that a regrouping of subjects is desirable in framing the school curriculum. This could be divided into four main divisions—English, Languages (ancient and modern), Science, Aesthetics. Science could be sub-divided into Mathematical Science (Mathematics, Physics, and Astronomy

and Natural Science (Biology, Chemistry, and Geology). In this case, mathematics and physics would be one subject and there could be considerable pruning of both elementary mathematics and elementary physics. The syllabuses of elementary examinations require far too much detail. A higher standard of fundamentals would be a better foundation for the future.

EPILOGUE

The pound weight is the unit of force used by engineers. Why do some teachers of mechanics develop the subject as though the pound mass is the engineers' unit of mass? This not only embarrasses engineers, but it gives rise to utter confusion in the use of other units and results in duplication of formulae.

There should be drastic and complete eradication of slovenliness in the use of words, units, formulae and an insistence on strict rigour of treatment. All teachers of dynamics should develop the idea of mass, and students should work problems in all units with equal facility.

All this can be achieved very simply, as follows:—

1. The terms mass and weight should, from the very beginning, be used carefully and correctly, with their accepted scientific meanings. The legal terminology of Acts of Parliament should be ignored.

2. A name is required for the gravitational unit of mass. Professor Worthington suggested the name slug.

3. Use appropriate standard notation and descriptions:

   $P=ma$ dynes, poundals, or pounds weight.
   Momentum $=mv$ gm.-cm./sec., lb.-ft./sec., slug-ft./sec.
   Kinetic energy $=\frac{1}{2}mv^2$ ergs, ft.-pdls., ft.-lbs. weight.
   Potential energy due to height $=Wh$ ergs, ft.-pdls., ft.-lbs. weight.
   Moment of inertia $=mk^2$ gm.-cm^2, lb.-ft^2, slug-ft^2
   Pressure $=P/A$ dynes/cm^2, pdls./ft^2, lbs. wt./ft^2
   Density $=m/V$ gm./c.c., lbs./c. ft., slugs/c. ft.

4. Use the same formulae and treatment for all systems of units and rigorously reject chaotic units. No duplication of formulae will arise.

APPENDIX 5

Treatment of the force equation in America *

The practice in America appears to be to teach the conception of mass more generally than in this country and, in consequence, the equation $F=ma$ is more universally used. There appears to be the same confusion of units and the same difference of opinion with regard to whether the poundal should be used or abolished. The slug appears to be more generally known than in this country.

* The whole of this article on mass, with the exception of Appendix 5, was written before consulting the American journals.

Another system of units is finding favour in certain quarters—the metre-kilogram-second system or M.K.S. system, in which the unit of force is the newton. 1 newton $= 10^5$ dynes.

Reference may be made to an article in the *American Physics Teacher* (August 1939, **7**, no. 4), "On the Meaning of a Constant in a Physical Law", by Prof. H. M. Dadourian. The article takes the form of a discussion between Simplicio, Sagredo, and Salviate. The topic for discussion is "the question of re-forming the equations of mathematics and the laws of physics along the lines laid down by certain authors of elementary physics text-books, who write the force equation as $F = kma$".

Most of the discussion relates to showing that k in this equation is an unnecessary constant of conversion. The only necessary constant in the equation is m, which is a constant of proportionality, which has dimensions, and which is a physical property of the body whose motion is considered.

The remarks on the poundal and the slug are vague and unsatisfactory and do not fit well into the argument.

"SIMP. I grant that your objections against conversion constants are well taken, so far as simplicity of equations and consistency of notation are concerned; but I maintain that there is something to be said for k in $F = kma$, for the poundal and for the slug on practical and pedagogic grounds. These enable students to solve problems that involve weight. I tell you it works.

SALV. There can be no practical use for them; otherwise they would be used in engineering and advanced physics. Who has ever heard weight spoken of as so many poundals, or so many slug-feet-per-second-per-second, outside of a classroom in elementary physics? Even in such classrooms these gewgaws are not mentioned after the first chapter or two. As for the pedagogic argument that 'it works', it works simply because problems are so worded that 'it' can't help working".

Later, Salviate makes the following suggestions:—

1. We can refrain from using the poundal and the slug.

2. We can refrain from using a superfluous k whose value jumps back and forth with values 1/981, 1/32, and 1.

3. We can stop talking about the "pound-force" or the "force-pound" and the "pound-mass" or the "mass-pound".

He now prescribes a remedy:—

"Let me show you a way which is not only theoretically sound but also very simple. Train the student to follow these rules:—1. Using letters, find an expression for the required quantity. 2. Perform the necessary literal substitutions until every letter in the expression represents a given or a known quantity. 3. Put the given numerical values in the expression after the second rule is complied with—never before. These are invaluable rules; they save time, reduce the number of numerical blunders and are aids to clear thinking. As an illustration of the simplicity of this method, suppose w and a are given, and F is required. The student writes $F = ma$; then, since m is not given, he looks for an equation by means of which he can eliminate it; he thinks of $w = mg$ and making use of it, obtains $F = wa/g$. The rest is a matter of mere substitution of numerical values. As simple as that."

The rules for solving a problem here prescribed completely beg the question. Students have no difficulty with literal formulae—the mistakes and the confusion in the solution of force problems arise entirely when numbers are put into the formulae. The poundal and the slug having been abolished, what numerical substitutions are necessary?

"SIMP. Then you favor writing w/g for mass, and $F/w=a/g$ or $F=wa/g$ for the force equation.

"SALV. Not at all. In fact, I object to these more than to $F=kma$, for the following reasons. The equation $F/w=a/g$ reduces a physical law, a very important one at that, to a mere numerical equation; for both F/w and a/g are pure numbers. On the other hand, $F=(w/g)a$ suffers from the disease of circularity, when considered as the definition of force. You will admit that a definition which expresses a force F in terms of another force w is not a legitimate definition. As to the use of w/g as the general definition of mass, there is little to be said for it. Authors who represent mass as w/g in the equations of physics ignore the fact that mass, being the most important property of matter, deserves a symbol of its own; they confuse a physical quantity with a particular way in which it may be measured under certain limited circumstances; they revert to geocentricism without being aware of it."

In the *American Journal of Physics* (April 1940, **8**, no. 2) there is a note by Professor W. W. Sleator with which I entirely agree. He says:—

"I should have agreed with Salviate in his defining equations, and in his definition of force in terms of mass and acceleration. But I should have objected to the condemnation of the poundal, and his bringing together 'the poundal and the slug' as if they both arose from the same objectionable equation.... He makes out an unfair case against the poundal, and by implication an unfounded charge of perversity or stupidity against those who use it.... Yet Dadourian will have none of the slug. Neither will he write $m=w/g$. He then has no English unit of mass at all..... Dadourian's general contention seems to be that we should use an absolute system of units, but, in order to conform to the usage of practical men, we should disguise the absolute unit in the result and express forces in pounds. This is one more variation on the common practice of placating the engineer by not trying to teach him anything.... We all teach a system of grams, centimeters, dynes, and ergs. To these correspond exactly the pound, foot, poundal and foot poundal. Two parallel systems are no more difficult to learn than one—perhaps less difficult. Why should the student be led to believe that in an English system one must form different units in a different manner?.... It might seriously be claimed that the poundal made things too easy. If this simple system makes physics too difficult for students, let them elect other subjects."

I entirely agree that two (and indeed three or four) parallel and rational systems of units are no more difficult to learn than one—perhaps less difficult. Using the simplest possible defining equation, $F=ma$, there are four rational parallel systems of units:—

	Mass	Length	Time	Force
C.G.S.	Gram	Centimetre	Second	Dyne
F.P.S.	Pound	Foot	Second	Poundal
British	Slug	Foot	Second	Pound-weight
M.K.S.	Kilogram	Metre	Second	Newton

Why foment confusion by the introduction of a formula $F/w=a/g$ for the propagation of chaotic units?

REVIEWS OF BOOKS

European Science, by H. STAFFORD HATFIELD. Pp. xiii+152. (Cambridge: The Basic English Publishing Company, 1939.) 3*s*. 6*d*.

This book has been written in basic English. As a medium for the interchange of ordinary thoughts and of limited ideas this language doubtless is of value. As a vehicle of scientific thought and ideas, however, it leaves much to be desired. For someone just learning to speak English, provided they learn the right 850 words (and that, with a little foresight, should not be difficult), this book will be of great value and interest. An English scientific reader will, however, probably be a trifle disappointed with the substance of the book. There are inconsistencies and statements which, from a scientific point of view, are inaccurate or incorrect. Thus, for example, it is stated: " The word ' weight ' has, even in science, kept its sense of Newton's ' mass ' ; and some of the best writers today are using it where mass would have been used some years back, for example, the ' weight of an electron '." ! Since *mass* appears to be allowed in basic—the word is used elsewhere in the book—it seems a pity, in a way, that the author has not catered for those of us who are unacquainted with the " best " writers. The statement that " Heat and light were now taken in " is, even in its context, rather surprising at first sight, and one is immediately brought face to face with the fact that basic English is not an ideal medium for scientific expression.

Another example of seeming inconsistency is an historical one. " Unhappily, Darwin's work was not straight away tested by experiment ; it was attacked by the Church .... While this was going on, one man, Mendel, was doing some simple experiments in his garden which were a test of the idea on which Darwin's theory was based ". The fact that Mendel was a unit of that Church, later abbot of the abbey in the garden of which these experiments were carried out, does not seem to have been appreciated.

W. B. M.

A Laboratory Manual of Electricity and Magnetism, by LEONARD B. LOEB. Pp. 121, with experimental sheets. (California: Stanford University Press; London: Sir Humphrey Milford, Oxford University Press, 1941.) 22*s*. 6*d*. net.

This book is a revised edition of a practical physics manual of electricity and magnetism for junior classes at the University of California, and is intended to supplement the author's *Fundamentals of Electricity and Magnetism*. The latest volume of the author is noteworthy for his unique method of instructing students, the salient features of which are clearly set out in the preface. In particular, Prof. Loeb stresses the importance of a student having at least some acquaintance with the relevant theory and basic principles of an experiment before he attempts to carry it out in the laboratory. Following this concept, the author, after titling an experiment and stating the objective to be achieved, gives an elementary description of its underlying theory. This procedure is, in the opinion of the writer, to be commended, especially in the case of large classes where it is impossible to arrange for all students to be performing experiments based on theory received in previous lectures. However, one feels that it is a somewhat

retrograde step to include, in the manual, perspective line diagrams of apparatus set up in electrical circuits.

In order to avoid students spending what the author considers an unnecessary length of time in writing-up laboratory records, there is incorporated at the end of each experiment a schematic table for the insertion of the quantitative results obtained. It is unfortunate from the students' point of view that the cost of the book is somewhat higher than it might be owing to the needless repetition (repeated four times in all) of these tables in the last hundred pages.

Twelve experiments are described in all, and they emphasize such fundamental concepts as magnetic and electric fields, electric current, potential difference, self-induction, etc. The terminology is in places a little confusing: for instance, on page 66 T_0 is used to signify the torsional coefficient of a galvanometer suspension, while T indicates its period ; again, English readers may be misled by the author's definition of the *Figure of Merit* of a galvanometer. The book is remarkably free from errors, except for one, of small consequence, at the top of figure 4, where F_M should read F_H.

R. W. B. S.

The Behaviour of Slow Electrons in Gases, by R. H. Healey and J. W. Reed. Pp. vii+169. (Sydney: Amalgamated Wireless (Australia), Ltd., 1941 ; agents for Great Britain: Iliffe & Sons, Ltd.) 20*s*.

A large number of original papers have been published during the past twenty-five years dealing with the study of the motion of slow electrons in gases by the diffusion methods originated by Townsend in Oxford and developed by Bailey in Sydney. Before the publication of this attractively produced book, which deals mainly with the results of these researches, both theoretical and practical, it would have been a difficult task for an enquirer to get a general view of the progress achieved.

An account of the fundamentals of electron motion in gases and of the early experimental work is given in Chapter 1. Chapters 2 and 3 are concerned with the formation of negative ions and with the velocity distribution of slow electrons in gases, respectively. Chapter 4 gives an account of results obtained by diffusion methods, while in Chapter 5 other methods of investigation are discussed and the results compared with those given by the diffusion method. There is a particularly full account of the formation of negative ions by attachment. Chapter 6—the last—deals with practical applications of the subject matter, following preliminary theoretical discussions of ionization by collision and the motion of electrons in gases under the influence of electric and magnetic fields. The effect of thunderstorms on the ionosphere, the possibility of production of artificial aurorae and the interaction of radio waves in the ionosphere, are all treated from both theoretical and practical aspects. The conclusion that it would be possible to produce artificial aurorae with the expenditure of only about 100 kw. of power (by using a suitable array of 4050 half-wave aerials transmitting at the local gyro-frequency) is most stimulating to the imagination.

Derivations of important formulae and very full experimental data are given throughout the text. References to individual papers are given conveniently in footnotes, as well as in the bibliography at the end of the book. It is claimed that this bibliography—relating to the whole field—is comprehensive.

The authors' aim to achieve completeness of discussion will make the book particularly useful for reference purposes ; the last chapter will certainly be of great interest to all concerned with the propagation of radio waves.

W. A. L.

Endeavour, Volume 1, Number 1, edited by E. J. HOLMYARD. Pp. 48. (Published by Imperial Chemical Industries, London, S.W. 1, January 1942.) 5*s*.

This first number of a new quarterly journal shows that it does not intend to trespass on the preserves of any existing publication. It is to survey scientific achievement, particularly British achievement, and to bring the results of this survey to as wide a world-audience as possible. For this reason it will be issued in French, German and Spanish editions, as well as in English.

The importance of good presentation and typography in a journal which may to some extent be judged as a sample of British workmanship has not been overlooked. Even more important than format is the choice of articles, and here the editor has been both discriminating and fortunate. The Astronomer Royal writes on the Sun's distance, now known to within 1 part in 10,000, Fairbrother on the Cyclotron, Newbury on the life and work of Glover, and Jacks on Soil Conservation. There are also biological articles by Waddington, Bacharach and Yonge, as well as general articles on Science in Britain and in the U.S.S.R. respectively, the former being beautifully illustrated with portraits of pioneers from Newton to Sir William Bragg.

In wishing the Journal the good luck which it merits, it would be unfair to the Directors of Imperial Chemical Industries not to express the gratitude of British men of science to their public-spirited action in launching the new venture. J. H. A.

Thermochemical Calculations, by RALPH R. WENNER. Pp. xii + 384. (New York and London: McGraw-Hill Book Co., Inc., 1941.) 28*s*.

" **Problem 33.** *The free energy of Gaseous Formaldehyde.*—Calculate the ΔF° of formation of HCHO(*g*) from the following data:—Heat of combustion = 134,100 cal./mole. Fundamental vibration frequencies = 1,165, 1,278, 1,503, 1,750, 2,780, 2,875 $cm.^{-1}$

Interatomic distances: C=O distance = 1·21 A.
C—H distance = 1·09 A.
H—C—H $\angle$ = 120°.

" **Problem 34.** *Electrolytic reduction of Nitrobenzene to Aniline.*—Estimate the kilowatt-hours required to produce 1 ton of aniline by cathodic reduction of nitrobenzene dissolved in strong acid. Assume a current efficiency of 80 per cent. and make reasonable assumptions as to over-voltages and cell resistance."

These two consecutive items from a list of sixty exercises given at the end of the book will give a clearer idea of what the student should learn from it than would a long description. In one sense it is an introduction to chemical engineering, but the outlook is far more theoretical than in any other book on this subject known to the reviewer. In another sense, it is a sequel to Lewis and Randall's standard text-book on *Thermodynamics*, but with a more practical outlook.

As a training for the reader who, in the author's words, has " already been exposed to some formal exposition of the principles of thermodynamics " it should be invaluable. He is taught to seek auxiliary data in the literature, to estimate missing data by logical comparison with data for similar compounds, to compute specific heats of gases from

spectroscopic data and to estimate compressibility data by making up an equation of state from the critical constants. In short, he is introduced to and encouraged to practise those tricks which he will have to use in later life if he is ever to make advances or to depart from established practice.

The last nine chapters deal with solubilities, titration and similar physico-chemical matters, chemical reactions, flame temperatures, converter design, gas-absorption plant, air conditioning, metallurgy, gas liquefaction, and fluid flow, each treated thermodynamically. The preceding eight chapters are provided as introductory to them, and deal with pure thermodynamics, and with methods of computing specific heats, enthalpies, Lewis free energy, and equilibrium constants. Lest the remark above, as to the calculation of specific heats from spectroscopic data, should be misleading, it should be added that the rotational and vibrational heat capacities of gases are also considered, and that solids are not overlooked.

The student brought up on these lines certainly starts his career with many advantages over his colleagues of the preceding generation. J. H. A.

The Cathode Ray Tube and its Applications, by G. PARR. Pp. viii + 180. (London: Chapman and Hall, Ltd., 1941.) 13*s*. 6*d*.

This well-produced second edition of *The Low Voltage Cathode Ray Tube*, by the editor of *Electronic Engineering*, gives a clear, well-informed and eminently readable account of the fundamental processes involved in the working of the cathode-ray oscillograph and of its many applications. In the preface, reference is made to the helpful criticism received from readers of the original edition of the book—and notably that from Mr. R. A. Watson-Watt. Although the author regrets that he could not describe some of the latest and best developments in cathode-ray technique, the book in its present compact and lucid form will undoubtedly prove most valuable to all readers who wish to obtain a general view of the field of cathode-ray oscillography. Detailed information which has been published on any particular branch of the subject will readily be obtained with the aid of the bibliography, which contains 737 references.

The range of the subject matter is so wide that the general reader might hesitate to voyage on such an ocean of specialized knowledge. However, the skill with which he is piloted here from the history, construction, operation and performance of cathode-ray tubes (chapter 1 and 2) through the intricacies of Lissajous' figures and time-base circuits (chapters 3, 4 and 5) to the diverse uses of the tube in radio engineering (chapter 6), in industry, and in many other applications, as, for example, in electromedical practice in studying heart-beats and brain-cell activity (chapter 7) to the final chapter on television reproduction, will amply repay his embarkation.

There are 80 figures, chiefly in the form of good line diagrams, and an Appendix on photography.

The book will serve as a most useful introduction to the subject for those who are preparing for future specialized training. W. A. L.